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*Wilhelm, A., ... Cornwall, Lebanon Co., Pa.
*Willard, H. B., ... Port Henry, Essex Co., N. Y.
*Williams, Prof. C. P., ... 113 Walnut Street, Philadelphia.
*Williams, Edward H., Jr., . . . Care Waterman & Co., Danville, Pa.
*Williams, Henry, ... Butte City, Montana.
*Williams, John R., ... Cambria Iron Co., Johnstown, Pa.
*Williams, John T., . Forty-fourth Street and East River, New York City.
†Williams, T. M., ... Wilkes-Barre, Pa.
*Wilson, John A., ... 410 Walnut Street, Philadelphia.

*Wilson, John L.,	439 Northampton Street, Easton, Pa.
*Wilson, John T.,	Wilson, Walker & Co., Pittsburgh, Pa.
†Winters, C. R.,	Rolla, Phelps Co., Mo.
*Wister, Jones,	Harrisburg, Pa.
*Witherbee, Frank S.,	Port Henry, Essex Co., N. Y.
†Witherbee, S. H.,	228 Madison Avenue, New York City.
*Witherbee, T. F.,	Port Henry, Essex Co., N. Y.
*Witherow, J. P.,	173 Wood Street, Pittsburgh, Pa.
*Wolf, Theodore G.,	Scranton, Pa.
*Wood, Edward L.,	2716 Carson Street, Pittsburgh, Pa.
*Wood, Frederick W.,	Steelton, Dauphin Co., Pa.
*Wood, Henry,	Streator, La Salle Co., Ill.
*Wood, Joseph,	Altoona, Pa.
*Woodward, Richard W.,	Ouray, Colorado.
†Woolson, O. C.,	35 Broadway, New York City.
†Wright, Harrison,	Wilkes-Barre, Pa.
*Wright, James N.,	Calumet, L. S., Michigan.
*Wrigley, Henry E.,	Titusville, Pa.
*Wurtz, Prof. Henry,	12 Hudson Terrace, Hoboken, N. J.
*Young, Chas. A.,	536 North Fourth Street, Philadelphia.
*Young, James B.,	Phœnix Roll Works, Pittsburgh, Pa.

Honorary Members, 5; Members, 610; Associates, 120; Foreign Members, 53.

Deceased.

Blossom, T. M.,	1876
Brown, A. J.,	1875
Clemes, J. P.,	1876
Daddow, S. H.,	1875
D'Aligny, H. F. Q ,	1875
Dresser, Charles A.,	1878
Firmstone, William,	1877
Gould, Robert H.,	1878
Harris, Stephen,	1874
Hunt, Thomas,	1872
Jenney, F. B.,	1876
Lee, Col. Washington,	1872
Liebenau, Charles von,	1875
Lord, John C.,	1872
Moore, Charles W.,	1877
Newton, Henry,	1877
Painter, Howard,	1876
Phelps, Walter,	1878
Richter, C. E.,	1877
Schirmer, J. F. L.,	1877
Steitz, Augustus,	1876
Stoelting, Hermann,	1875
Walz, Isidor,	1877
Witherbee, J. G.,	1875

RULES.

ADOPTED MAY, 1873. AMENDED MAY, 1875, MAY, 1877, AND MAY, 1878.

I.

OBJECTS.

The objects of the AMERICAN INSTITUTE OF MINING ENGINEERS are to promote the Arts and Sciences connected with the economical production of the useful minerals and metals, and the welfare of those employed in these industries, by means of meetings for social intercourse, and the reading and discussion of professional papers, and to circulate, by means of publications among its members and associates, the information thus obtained.

II.

MEMBERSHIP.

The Institute shall consist of Members, Honorary Members, and Associates. Members and Honorary Members shall be professional mining engineers, geologists, metallurgists, or chemists, or persons practically engaged in mining, metallurgy, or metallurgical engineering. Associates shall include all suitable persons desirous of being connected with the Institute and duly elected as hereinafter provided. Each person desirous of becoming a member or associate shall be proposed by at least three members or associates, approved by the Council, and elected by ballot at a regular meeting upon receiving three-fourths of the votes cast, and shall become a member or associate on the payment of his first dues. Each person proposed as an honorary member shall be recommended by at least ten members or associates, approved by the Council and elected by ballot at a regular meeting on receiving nine-tenths of the votes cast; *Provided*, that the number of honorary members shall not exceed twenty. The Council may at any time change the classification of a person elected as associate, so as to make him a member, or *vice versâ*, subject to the approval of the Institute. All members and associates shall be equally entitled to the privileges of membership; *Provided*, that honorary members, and members and associates permanently residing in foreign countries, shall not be entitled to vote or to be members of the Council.

Any member or associate may be stricken from the list on recommendation of the Council, by the vote of three-fourths of the members and associates present at any annual meeting, due notice having been mailed in writing by the Secretary to the said member or associate.

III.

DUES.

The dues of members and associates shall be ten dollars per annum, payable in advance at the annual meeting; *Provided*, that persons elected at the meeting following the annual meeting shall pay eight dollars, and persons elected at the meeting preceding the annnal meeting shall pay four dollars as dues for the current year; and members and associates permanently residing in foreign countries, excepting Canada, shall be liable to such annual or other payments only as the Council may impose, to cover the cost of supplying them with publications. Honorary members shall not be liable to dues. Any member or associate may become, by the payment of one hundred dollars at any one time, a life member or associate, and shall not be liable thereafter to annual dues. Any member or associate in arrears may at the discretion of the Council be deprived of the receipt of publications, or stricken from the list of members when in arrears for one year; *Provided*, that he may be restored to membership by the Council on payment of all arrears, or by re-election after an interval of three years.

IV.

OFFICERS.

The affairs of the Institute shall be managed by a Council, consisting of a President, six Vice-Presidents, nine Managers, a Secretary and a Treasurer, who shall be elected from among the members and associates of the Institute at the annual meetings, to hold office as follows:

The President, the Secretary, and the Treasurer for one year (and no person shall be eligible for immediate re-election as President who shall have held that office subsequent to the adoption of these rules, for two consecutive years), the Vice-Presidents for two years, and the Managers for three years; and no Vice-President or Manager shall be eligible for immediate re-election to the same office at the expiration of the term for which he was elected. At each annual meeting a President, three Vice-Presidents, three Managers, a Secretary and a Treasurer shall be elected, and the term of office shall continue until the adjournment of the meeting at which their successors are elected.

The duties of all officers shall be such as usually pertain to their offices, or may be delegated to them by the Council or the Institute; and the Council may in its discretion require bonds to be given by the Treasurer. At each annual meeting the Council shall make a report of proceedings to the Institute together with a financial statement.

Vacancies in the Council may occur by death or resignation; or the Council may by vote of a majority of all its members declare the place of any officer vacant, on his failure for one year, from inability or otherwise, to attend the

Council meetings or perform the duties of his office. All vacancies shall be filled by the appointment of the Council, and any person so appointed shall hold office for the remainder of the term for which his predecessor was elected or appointed; *Provided*, that the said appointment shall not render him ineligible at the next annual meeting.

Five members of the Council shall constitute a quorum; but the Council may appoint an Executive Committee, or business may be transacted at a regularly called meeting of the Council, at which less than a quorum is present, subject to the approval of a majority of the Council, subsequently given in writing to the Secretary, and recorded by him with the minutes.

V.

ELECTIONS.

The annual election shall be conducted as follows: Nominations may be sent in writing to the Secretary, accompanied with the names of the proposers, at any time not less than thirty days before the annual meeting; and the Secretary shall, not less than two weeks before the said meeting, mail to every member or associate (except honorary members, or foreign members or associates), a list of all the nominations for each office so received, stamped with the seal of the Institute, together with a copy of this rule, and the names of the persons ineligible for election to each office. And each member or associate, qualified to vote, may vote, either by striking from or adding to the names of the said list, leaving names not exceeding in number the officers to be elected, or by preparing a new list, signing said altered or prepared ballot with his name, and either mailing it to the Secretary, or presenting it in person at the annual meeting: *Provided*, that no member or associate, in arrears since the last annual meeting, shall be allowed to vote until the said arrears shall have been paid. The ballots shall be received and examined by three Scrutineers, appointed at the annual meeting by the presiding officer; and the persons who shall have received the greatest number of votes for the several offices, shall be declared elected, and the Scrutineers shall so report to the presiding officer. The ballots shall be destroyed, and a list of the elected officers, certified by the Scrutineers, shall be preserved by the Secretary.

VI.

MEETINGS.

The annual meeting of the Institute shall take place on the third Tuesday of February, at which a report of the proceedings of the Institute, and an abstract of the accounts, shall be furnished by the Council. Two other regular meetings of the Institute, shall be held in each year, at such times and places as the Council shall select. Special meetings may be called whenever the Council sees fit; and the Secretary shall call a special meeting on a requisition signed by fifteen or more members. The notices for special meetings shall state the business to be transacted, and no other shall be entertained. All notices may be given by circular, mailed to members and associates, or through the Bulletin, published in the regular organ of the Institute, at the discretion of the Council

Every question which shall come before any meeting of the Institute, shall be decided, unless otherwise provided by these Rules, by the votes of the majority of the members then present. The place of meeting shall be fixed in advance by the Institute, or, in default of such determination, by the Council, and notice of all meetings shall be given by mail, or otherwise, to all members and associates, at least twenty days in advance. Any member or associate may introduce a stranger to any meeting; but the latter shall not take part in the proceedings without the consent of the meeting.

VII.

PAPERS.

The Council shall have power to decide on the propriety of communicating to the Institute any papers which may be received, and they shall be at liberty, when they think it desirable, to direct that any paper read before the Institute, shall be printed in the Transactions. Intimation, when practicable, shall be given at each General Meeting, of the subject of the paper or papers to be read, and of the questions for discussion at the next meeting. The reading of papers shall not be delayed beyond such hour as the presiding officer shall think proper; and the election of members or other business may be adjourned by the presiding officer, to permit the reading and discussion of papers.

The copyright of all papers communicated to, and accepted by the Institute, shall be vested in it, unless otherwise agreed between the Council and the author. The author of each paper read before the Institute shall be entitled to twelve copies, if printed, for his own use, and shall have the right to order any number of copies at the cost of paper and printing, provided said copies are not intended for sale. The Institute is not, as a body, responsible for the statements of fact or opinion, advanced in papers or discussions, at its meetings, and it is understood that papers and discussions should not include matters relating to politics or purely to trade.

VIII.

AMENDMENTS.

These Rules may be amended, at any annual meeting, by a two-thirds vote of the members present, provided that written notice of the proposed amendment shall have been given at a previous meeting.

PROCEEDINGS

OF THE

CHATTANOOGA MEETING.

MAY, 1878.

THE Institute met on Wednesday evening, May 22d, in the parlor of the Stanton House, Dr. T. Sterry Hunt, President, in the chair.

The President delivered an introductory address on the Brown Hematite Deposits of the Great Valley, after which the following papers were read and discussed:

Note on the Result of an Experiment with the Wheeler Process of Combining Iron and Steel in the Head of a Rail, by W. E. C. Coxe, of Reading, Pa.

On the Jenks Corundum Mine, Macon County, N. C., by Dr. R. W. Raymond, of New York City.

On Wednesday afternoon, previous to the opening session, the members who had already arrived in Chattanooga, were taken by the Local Committee to the works of the Roane Iron Company, and Chattanooga Iron Company, and then to the top of Lookout Mountain, where, at the request of the members present, Dr. Hunt described the geological and geographical features of the Chattanooga region, with the magnificent panorama spread out before them as a map.

On Thursday morning an excursion was made by steamboat down the Tennessee River as far as Shellmound. Here cars were taken to the coal mines of the Dade Coal Company, where the members were courteously received by Mr. E. B. Wells, the Superintendent. After a trip through the mines the party partook of a collation provided by the company. South Pittsburgh, on the Tennessee River, was next visited, where the Southern States Coal, Iron and Land Company have a large blast-furnace plant in the course of erection. After conducting the party through the works, shops, etc., Mr. James Bowron, the manager, entertained the members with graceful hospitality at dinner.

The second session was held on Thursday evening for the transaction of business.

The President, Dr. T. Sterry Hunt, appointed Messrs. W. E. C. Coxe, R. H. Richards, and F. J. Slade, scrutineers, to examine the ballots for officers for the ensuing year.

The following persons proposed for membership in the Institute, and approved by the Council, were unanimously elected:

MEMBERS.

Truman H. Aldrich,	Montevallo, Shelby Co., Ala.
S. W. Baldwin,	Yonkers, N. Y.
James C. Bayles,	New York City.
James Bowron,	South Pittsburgh, Tenn.
Thomas M. Carnegie,	Pittsburgh, Pa.
Gaylord B. Clark,	Mobile, Ala.
Erastus Corning,	Albany, N. Y.
Edward Dowd,	Chattanooga, Tenn.
James M. Duncan,	Chattanooga, Tenn.
Charles M. Dupuy,	Philadelphia.
William Goodnow,	Atlanta, Georgia.
Henry C. Freeman,	Alto Pass, Ill.
John H Hillman,	Tennessee Rolling Works, Ky.
Thomas T. Hillman,	Tennessee Rolling Works, Ky.
B. F. Jones,	Pittsburgh, Pa.
Robert Kyle,	Atlanta, Georgia.
George Lauder,	Larimer Station, P. R. R., Pa.
F. B. Laughlin,	Pittsburgh, Pa.
Selden E. Marvin,	Albany, N. Y.
Dwight P. Montague,	Chattanooga, Tenn.
Henry B. Richmond,	Philadelphia.
Stephen P. Sharples,	Boston, Mass.
F. I. Stone,	Chattanooga, Tenn.
Adolph Thies,	Bowden, Carroll Co., Georgia.
Willard Warner,	Tecumseh, Ala.
Amos G. West,	Cedartown, Polk Co., Georgia.
Carl Zogbaum,	Germantown, Philadelphia.

ASSOCIATES.

Joseph M. Alexander,	Jacksonville, Ala.
George H. Frost,	Chicago, Ill.
Samuel B. Lowe,	Chattanooga, Tenn.
W. P. Rathburn,	Chattanooga, Tenn.
Oliver P. Scaife,	Pittsburgh, Pa.

It was also voted that the status of Messrs. F. R. Hutton and J. R. Priest be changed from associate to member.

The following Report of the Council was then read:

The Council, in accordance with the rules, makes the following report to the Institute of the work of the past year:

There has been an accession to the membership of seventy-one,—fifty members, eighteen associates and one foreign member. Twelve have resigned and four have died, leaving a total membership at present of seven hundred and forty. The members whose death the

Institute mourns are William Firmstone, Henry Newton, Chas. A. Dresser, and C. W. Moore.

The three regular meetings of the Institute held at Wilkes-Barre, Amenia, and Philadelphia were highly profitable in valuable papers contributed, in excursions to points of interest, and in social enjoyment. The Council notes with gratification the large and increasing attendance at the regular meetings and the expressions of pleasure of the members who attend them.

The fifth volume of *Transactions* has been issued, covering the period from May, 1876, to February, 1877, and has been distributed to all foreign members, and to home members and associates not in arrears for annual dues. The volume has also been distributed to scientific and technical societies in this country and in Europe, and also to general libraries. The library of the Institute is steadily increasing in value by exchanges received for the *Transactions*. The full analytical index to the five volumes which has been added to Volume V, will be found of great convenience in consulting these volumes.

Owing to unavoidable delays in issuing Volume V, the publication of papers in pamphlet form, as determined upon at the Amenia meeting, has not been as prompt as was expected. Four have, however, already been issued, and the remainder will now speedily follow, and members will have an opportunity to judge of the desirability and convenience of this system of publication.

The reports of the Secretary and Treasurer of the finances of the Institute, duly audited, show receipts for the year of $4828.99, and expenditures of $4748.16, leaving a balance of $80.83. There are liabilities of $476.87, and a net deficit, therefore, of $396.04.*

The Institute has received communications from the Society of German Engineers, the Society of Civil Engineers of France, and from the Prussian Minister of Commerce, Industry, and Public Works, extending courtesies to members who may visit the Paris Exhibition this summer. The Society of German Engineers has appointed committees in the different districts of Germany to receive members, and aid them in their investigations of German industries by information and introductions. The Society of Civil Engineers of France offers its libraries, halls, and rooms in Paris to members for study, meetings, or rendezvous, and the Prussian Minister has established a bureau of information at Berlin, where members can resort for inquiries and instructions.

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* Detailed statement in Vol. VI, Transactions.

These courtesies, offered in the most liberal and cordial manner, are the more gratifying, inasmuch as they are, in a measure, an acknowledgment of the facilities which the Institute had the pleasure of offering to foreign engineers on the occasion of the Exhibition in Philadelphia.

Mr. J. M. Alexander, of Jacksonville, Alabama, presented invitations from the Board of Trade of Mobile and from the Mayor of Jacksonville, Alabama, requesting the Institute to hold one of its future meetings in Mobile. The matter was referred to the Council, and the thanks of the Institute expressed to Mr. Alexander for the cordial invitation.

The proposed amendments to the Rules were then considered. Mr. Holley's amendments referring to the times of the annual and other meetings, and also the amount of dues payable by members on election at each meeting, were adopted in the following form:

Rule VI to read: The annual meeting of the Institute shall take place on the third Tuesday of February, at which a report of the proceedings of the Institute and an abstract of the accounts shall be furnished by the Council. Two other regular meetings of the Institute shall be held in each year at such times and places as the Council shall select. Special meetings may be called, etc., etc.

Rule III to read: The dues of members and associates shall be ten dollars per annum, payable in advance at the annual meeting, *provided*, that persons elected at the meeting following the annual meeting shall pay eight dollars, and persons elected at the meeting preceding the annual meeting shall pay four dollars as dues for the current year; and members and associates permanently residing in foreign countries, excepting Canada, etc., etc.

The motion to adopt the amendments proposed by Prof. Prime to Rules II and III, referring to members and associates permanently residing in foreign countries, was laid on the table. Mr. Firmstone moved that his proposed amendment to Rule II, referring to the method of electing members and associates, the consideration of which was postponed at the Wilkes-Barre meeting, be again postponed until the next annual meeting.

The motion was carried.

Dr. Raymond offered the following resolution, which was adopted:

Resolved, That the Council be and is hereby authorized to incorporate itself as the American Institute of Mining Engineers, in such corporate capacity to adopt the present existing rules, elect the present members and associates, collect the dues unpaid, continue in all respects the functions of the present association, and

that upon the consummation of such action by the Council the present unincorporated association, known as the American Institute of Mining Engineers, shall be merged in the incorporated association of the same name so formed.

Prof. Egleston, Chairman of the Committee on Gauges, moved, in view of the fact that the subject had been taken up by the Metrological Society, that the committee be discharged, which was agreed to.

The following papers were then read by title:

The Mode of Combustion in Blast-Furnace Hearths, by Prof. John A. Church, of Columbus, Ohio.

Memorandum Relating to the Construction Account of the Rail Mill of the Edgar Thomson Steel Company, Pittsburgh, Pa., 1874 and 1875, by P. Barnes, of Plainfield, N. J.

The Geology as Related to the Mineralogy of the Chattanooga Mineral Region, by General J. T. Wilder, of Chattanooga, Tenn.

Blast-Furnace Notes and Statistics, by C. Constable, of Rockwood Tenn.

The Humboldt-Pocahontas Vein, Rosita, Colorado, by R. Neilson Clark, of Rosita, Colorado.

Thin Sheets of Metal, and Examinations of Iron and Steel Rails, by Prof. T. Egleston, School of Mines, New York City.

On a Deposit of Cadmia in a Coke Blast Furnace, by H. Firmstone, of Longdale, Allegheny Co., Va.

The following resolution of thanks was then unanimously passed:

Resolved, That the hearty thanks of the American Institute of Mining Engineers be given to the following individuals, associations, and companies, who have so cordially received its members and extended to them, during the Chattanooga meeting, thoughtful courtesy and graceful hospitality, and especially to Messrs. H. S. Chamberlain, J. T. Wilder, Mayor Carlisle, the local Committees of Arrangements, and also to the ladies of Chattanooga, whose exertions for the pleasure and profit of the members of the Institute during this delightful meeting have contributed so largely to its success: The Iron, Coal and Manufacturers Association of Chattanooga; Roane Iron Company; Chattanooga Iron Company; Tennessee Iron and Steel Company; Vulcan Iron and Nail Works; Dade Coal Company, Mr. E. B. Wells, Superintendent; Southern States Coal, Iron and Land Company, Mr. James Bowron, Manager; Mr. C. Constable, Manager of Rockwood Furnace and Mines; Eureka Company, Mr. James Thomas, Manager; Shelby Iron Company, Mr. Black, Manager; Woodstock Iron Company, Mr. A. L. Tyler, President; Nashville, Chattanooga and St. Louis Railroad Company, Mr. J. W. Thomas, General Superintendent; Alabama and Chattanooga Railroad Company, Mr. Charles B. Ball, General Superintendent; Western and Atlantic Railroad Company, Mr. William MacRae, Superintendent; South and North Alabama Railroad Company; Selma, Rome and Dalton Railroad Company, Mr. M. Stanton, General Superintendent; East Tennessee,

Virginia and Georgia, Memphis and Charleston, and Knoxville and Ohio Railroad Companies, Mr. C. M. McGhee, Vice-President; Captain Capehart, of the Steamer R. C. Jackson, and Captain Kendrick, of the Steamer J. T. Wilder.

On Thursday evening the members left Chattanooga under the guidance of Mayor Carlisle by the Alabama and Chattanooga Railroad, and arrived at Birmingham, Alabama, at six o'clock Friday morning, thence by the South and North Alabama Railroad to Oxmoor, where breakfast was provided by Mr. James Thomas, Manager of the Eureka Company. Visit was then made to the company's mine on Red Mountain by narrow-gauge road two and a half miles long, where workings in fossil ore were seen, and also the inclined plane to bring limestone over the mountain to the railroad. Returning to Oxmoor, the furnaces and coke ovens were inspected. The party then proceeded on the South and North Railroad to the company's coal mines at Helena in the Cahaba coal field, and examined the Stutz coal-washer and the coke ovens, thence to the junction of the Selma, Rome and Dalton Railroad at Calera, and over this road to the Shelby Iron Works, Mr. Black, Superintendent, where the charcoal furnaces and extensive brown hematite deposits were seen and the party entertained at dinner. Continuing on the Selma, Rome and Dalton Railroad, the Woodstock furnaces and mines were next visited, under the guidance of Mr. Alfred L. Tyler, President of the company, who also hospitably provided the party with supper. Continuing on the above road as far as Dalton, the party arrived in Chattanooga early Saturday morning, via the Western and Atlantic Railroad.

The third and concluding session of the Institute was held on Saturday morning, Vice-President Raymond in the chair.

Mr. H. S. Chamberlain presented a number of copies of a pamphlet on "The Agricultural and Mineral Wealth of Tennessee," by J. B. Killebrew, and lithographic copies of the large centennial map of the Chattanooga region for distribution to members. The secretary called attention to convenient cards for the interconversion of English and metric units, prepared by Prof. Persifor Frazer, Jr., of Philadelphia, and sent by him for distribution.

The following papers were then read:

A New Steam-Engine Indicator, by J. E. Sweet, Cornell University, Ithaca, N. Y.

Heat in the Comstock Lode and its Cause, by Prof. John A. Church, of Columbus, Ohio.

Improvements in Appliances for Venting Molten Steel or Iron from a Casting Ladle or Shoe, by J. A. Herrick, of Nashua, N. H.

Note on Zircons in Unaka Magnetite, by Prof. W. P. Blake, of New Haven.

The scrutineers appointed by the President to examine the ballots reported the following persons elected:

PRESIDENT.

ECKLEY B. COXE,	Drifton, Luzerne Co., Pa.

VICE-PRESIDENTS.

J. F. BLANDY,	Merchantville, N. J.
WILLIAM METCALF,	Pittsburgh, Pa.
R. H. THURSTON,	Hoboken, N. J.

MANAGERS.

GEORGE ASMUS,	New York City.
B. W. FRAZIER,	Bethlehem, Pa.
WILLIAM B. POTTER,	St. Louis, Mo.

TREASURER.

THEODORE D. RAND,	Philadelphia.

SECRETARY.

THOMAS M. DROWN,	Easton, Pa.

The meeting was then declared adjourned.

On Saturday evening the members left Chattanooga by boat for Rockwood, Tennessee, to visit the Roane Iron Company's blast furnace and coal mines. They were here received by General J. T. Wilder, and Mr. C. Constable, the Manager, who conducted the party over the works and afterwards, with the graceful assistance of the ladies of Rockwood, entertained them with generous hospitality at dinner. On leaving Rockwood, the trip on the Tennessee River was continued to Loudon, where a special train conveyed the members to Knoxville. Here they were made the guests of Mr. C. M. McGhee, Vice-President of the East Tennessee, Virginia and Georgia Railroad, who took them over the Knoxville and Ohio Railroad to the Coal Creek coal mines. On returning to Knoxville they were taken in carriages to the environs of the city, where extensive views of the surrounding country were obtained. This concluded the excursions of the meeting, the party here breaking up and dispersing to their homes.

PAPERS

OF THE

CHATTANOOGA MEETING.

MAY, 1878.

IMPROVEMENTS IN THE APPLIANCES FOR VENTING MOLTEN STEEL OR IRON FROM A CASTING-LADLE OR SHOE.

BY J. A. HERRICK, NASHUA, N. H., SUPERINTENDENT STEEL SMELTING DEPARTMENT, NASHUA IRON AND STEEL COMPANY.

In this country steel made in a Siemens furnace or Bessemer converter, is generally tapped into a ladle or shoe, and then drawn through an aperture in its base into the various moulds.

In order to regulate the flow of the molten mass a refractory valve is arranged at the aperture. A plumbago nozzle is fixed firmly in this aperture, and a stopper of like material is fitted to it, making a socket valve. In order to raise and depress this stopper at will, a rod is fastened to it, extending through the metal and over the brim of the ladle, where it is bent back in a half circle and secured in a slide. This slide plays vertically in a frame, permanently fastened to the outside of the ladle, and is moved by a lever having as a fulcrum a pin fixed in this frame, one end of the lever being temporarily fixed to the slide, and the other end being moved by hand. Reference being had to the drawing (see Plate 1, Fig. 1), E is the nozzle, D is the stopper, A is the rod, protected as hereinafter described, C is the cast-iron framework, B is the wrought-iron slide, B′ is the offset into which the rod, A, enters, and is secured in the usual manner by a key, a. Section J, K, shows the manner of hanging the lever. J is a pin attached to the slide by which the end of the lever raises the slide, B. J is the fulcrum fastened to the frame, C. K (Fig. 1) is a hand-nut fastening the slide, B, to the frame, C, at will. Fig. 2 is a side elevation of ladle, showing the outside working of the parts above described.

It is the common practice to protect the rod, A, by a coating of clay and fire-sand, kneaded to the right consistency and applied to the rod, and then thoroughly dried. Just before casting, the plumbago stopper, D, is heated at its extremity to redness separate from the ladle and, at the moment of pouring, is placed in the ladle and slide, and adjusted as well as practicable. Some very serious defects ensue, causing much trouble and vexation as well as actual loss. The fire-clay coating in drying becomes cracked and seamy, and at its best is more or less loose on the rod. The hot metal sometimes cuts

through this coating, severing the central rod; and in order to draw out the remainder of the metal the stopper, now fallen and resting on the nozzle, must be punched up from beneath—a very undesirable operation; or else the ladle must be reversed and the metal, mixed with slag, poured over the brim. Any jar is likely to detach portions of the coating, and the above-mentioned casualty ensues. Under the most favorable conditions this kind of protection of the rod must be renewed for each heat, none being fit for more than one pouring. The whole stopper must necessarily be imperfectly heated outside of the ladle, and then, while comparatively cold, must be fitted into the slide, the *ladle itself becoming rapidly cooled* during this operation. The stopper being unevenly heated the centre rod is sometimes warped, the parts of the valve do not fit together perfectly, and a leakage of metal while transferring the ladle from one ingot mould to another very often occurs.

Thus managed, a stopper is necessarily much colder than the ladle, and when the molten metal strikes the former, an ebullition more or less violent ensues, causing much personal inconvenience, and tending to make the ingots porous, imperfect, and of light weight. The surge of the molten metal wears off portions of the clay coating, and these are frequently found in the completed steel as sand-streaks. The above-mentioned and other defects, more or less serious in their nature, and which will occur at once to any expert, are almost entirely corrected by the improvements about to be described. These have been in actual use for the last four months in a new twelve-ton ladle. In a six-ton ladle the above-named evils are hard to endure, but are much intensified as the size of the ladle increases.

The fire-clay coating is done away with, and one or more cylinders of refractory material, M (see details of stopper), are substituted, covering the rod as high as the metal and slag rises. In order to prevent the steel or iron from penetrating the joints of the cylinders and injuring the inclosed rod, a socket is arranged between the cylinders as at N. An angular projection on the first cylinder fits into a square-shouldered cavity in the second, thereby breaking joints and preventing a thin film of metal from reaching the rod. The plumbago stopper, D, is arranged with a projection upon its top face, which enters the first cylinder, as shown in the detailed drawing. The stopper is fastened to the rod, in the ordinary way, by a pin with a shoulder at its extremity passing up through the stopper, while the other end of the pin, also passing up through the

lower part of the rod, is fastened by a wedge, L, as shown in detail. The cylinders when in use expand and contract, making the joints open and close, and exposing in some degree the inclosed rod. To obviate this a stiff spring, O, is placed on the top of the cylinders, and put under proper tension by the wedge, Q, and driven to a bearing through a slot in the rod. To protect this spring from the heat, flying steel, or overflowing slag, a thin cap or hood, P, of cast iron or any refractory material is arranged, completely protecting it. The outside of this cap or hood is painted thickly with black lead and clay, mixed thin with sour beer, each time the stopper is used. In order to readily adjust this stopper, even while actually pouring, a hinge motion is arranged on the side at F, and in order to work the hinge and at the same time keep it rigid, a metallic bar, G, Fig. 1, about two inches wide by one-half inch thick, is bolted firmly at H to the offset, while the lower end of the bar swings free.

A rod or bolt, I, projects at right angles from the main slide, B, with a thread cut along its surface. A hand-nut is placed on this rod outside of the connecting bar, G, while a check-nut easily turned is placed on the inside of the bar, thus checking motion at any desired point, and making the whole stopper rigid. Motion towards the opposite side of the ladle in the plane of its centre is thus secured. A rotary motion of stopper and rod, A, is secured by loosening the key, a, and simply turning the rod in its socket. A set screw turned by hand fixes the rod in place, and prevents further rotation, while the key, a, driven home, secures rigidity. The above improvements can readily and with small expense be adapted to the ordinary slides of ladles. Great comfort, ease, and certainty, are thus secured in the pouring of steel or other fluid metal; much labor and even money is saved, for these stoppers last several heats without change or trouble, the stopper remaining in place, and being heated in its working position in the ladle.

DISCUSSION.

Mr. Holley remarked that the results obtained by Mr. Herrick were unusually good. The fire-brick sleeves on the stopper-rod are not new; they have been used for several years in the Bessemer practice. But the spring to compensate for the expansion of the rod is novel and important. The necessity for such a compensation is so great that at Seraing, in Belgium, a device was originated which was

copied in many foreign and American works,—a piece of paper under the pin which holds a solid-ended stopper to the rod. When the stopper gets hot, the paper burns away, giving the rod a chance to expand. Mr. Herrick's arrangement appears to be much better.

A NEW STEAM-ENGINE INDICATOR.

BY JOHN E. SWEET, ITHACA, N. Y.

THERE have already been so many subjects of a purely mechanical nature presented to the Institute of Mining Engineers, that it is unnecessary for me to apologize for adding another to the list.

When a new invention, or what is supposed to be a new invention, claiming simply an improvement on the well-known devices, is to be described, it is usual to point out the defects of the old and give a history of the object or the art up to date. But, as no doubt a large proportion of the members of this Institute know as much, or more, about the history and use of the steam-engine indicator than I do myself, I will not further preface the subject, than to remind you that what called for the Richards's improvement on the Watt's indicator was high speed; that the leading improvement was the reduction of momentum in the reciprocating parts; and that this was accomplished by making the parts lighter, and reducing the travel of the heaviest of them, and consequently the speed and momentum.

The use of the indicator has been extended, for the speed of steam-engines and other prime movers has been increased, until one engineer at least, John Cooper, of Philadelphia, says, "I want an indicator that will work up to a thousand revolutions."

Mr. Thompson, of the Buckeye Steam-Engine Company, of Salem, Ohio, has improved on the Richards's indicator by another reduction in the momentum, which is done by putting the parallel motion nearer the fulcrum of the pencil arm, reducing both weight and travel.

The characteristic feature of the indicator I am going to illustrate is a still further reduction in the same direction, which is accomplished by dispensing with the parallel motion altogether. As is often the case, in putting a simple idea into shape, some difficulties

were encountered that had to be overcome, and new thoughts and improvements presented themselves, so that the original design has been almost overshadowed by other ideas. I will describe the whole apparatus in almost the reverse order in which the various changes have been thought out.

Figures 1 and 2 (Plate II) show the reducing device we have applied to our engine for giving motion to the paper. A, Fig. 1, is a crooked rock shaft mounted in bearings over the cross-head of the engine, in the supports, B, B. C, a pendulum-rod, swung back and forth by the motion of the cross-head, carries a sliding block, D, which can be raised to the top of the rod or allowed to fall to any desired point by the adjusting collar, E. A string, F, passing through the centre of the rock shaft and over the pulley enables the operator to raise or lower this block regardless of the speed of the engine. The trunnions on the block are, of course, at rest when the block is raised so that their axis corresponds with the axis of the rock shaft, and, when let down, have a motion proportional to the motion of the cross-head. We suspend a weight somewhat heavier than the block and its connections to the cord, so that by raising the weight we give motion to the indicator, or by dropping the weight stop it. This arrangement is applicable to all indicators. It is especially good for high speed, and there is no objection except the one of cost, and that, if we consider the unsightly and frail traps that are frequently adopted, will tend to make us think more instead of less of it. Of course, this arrangement would need modifying to suit circumstances, but the principle of sliding the block to and from the axis of motion is applicable in all cases. In the place of a string for connecting the reducing device with the indicator, we use a connecting rod, one end of which is shown at *a*, Fig. 2.

Figures 3 and 4 show the indicator in cross section and elevation. It has been built, as you will see, upon the Richards's indicator, enough of the original being used to make it recognizable; in fact, we used our Elliott Bros. instrument, altering it only so far as was necessary to carry out the idea.

It will be noticed that the principal difference is in the method of holding and moving the paper. Instead of wrapping it around the outside of a cylinder, it is bent and held on the inner surface of a segment; and, instead of being moved around and back in a circle, it is moved forward and back in a straight line. So the power from the engine is carried directly to the paper-holder, without strings or springs, and is a positive motion which can be run at any speed.

This method of fixing the paper is better in two respects: it can be placed in one-fourth the time, and any kind of paper or stiff business card can be used. To place the card it is only necessary to set the lower edge in the bottom channel, press it back, and adjust the hook G over the upper edge.

The pencil-arm is a tube of about $\frac{1}{16}$th of an inch external diameter, through the centre of which passes the marking-point, of copper wire, about $\frac{1}{32}$d of an inch in size. I am indebted to John Cooper for the idea of using copper. I had contemplated using zinc or some alloy, but copper works so well that I have tried no other point. The pressure is put on the marking-point by the spring, H, which is forced against it by the cam, I. In this way the pressure is put on, and the force is the force of the spring, entirely beyond the control of the operator.

The first difficulty which presented itself to us, and one which may have already occurred to some of you, was: how is a uniform travel of the piston to be made to give a uniform travel to the pencil-point? This has been beautifully accomplished by the method shown in Fig. 5, which was devised or discovered by Mr. F. A. Halsey, the student who has made the indicator itself and the drawings of it.

It was thought best to give the pencil the same range as in the old indicator,—60°; but, instead of moving 30° above and 30° below, and locating the piston and link in a natural position, the arm plays from 7½° below to 52½° above the horizontal, and the link is placed in that inclined position which gives to the pencil so nearly a uniform velocity ratio that when multiplied twelve-fold the variation is scarcely discernible. I have given in an appendix, in Mr. Halsey's own words, a mathematical consideration of this problem and an explanation of its solution. It will be seen that we not only dispense with the parallel motion, but do away with considerable weight in the pencil and pencil-holder at the end of the arm, where it does the most harm.

So far as we have been able to test the indicator, the result has been to get as good cards at 330 revolutions as we could get from the Elliot-Richards at 220, or from the Thompson at 270, under the same conditions; and 330 is the highest speed at which we have had an opportunity of trying it.

One objection to the new instrument is that it is a trifle more bulky; but it is no heavier, and, I think, somewhat less in cost.

During the construction of this instrument another question came

up, and that was whether it was not possible to do away with the most of the piston-rod and all the weight of the spring by substituting a torsion for the spiral spring. Figures 6 and 7 show this idea developed; and this has led to two or three other ideas that would seem to justify the carrying out of the experiment. It is proposed to use two torsion springs, A and B, each of square wire, one to be about equal to a 30-pound spring and the other to a 60-pound spring. For low pressure the 30-pound spring can be used, for high pressure the 60-pound, and for still higher both. Then, again, for condensing-engines, the 60-pound spring for the high pressure, and the 30-pound at the same time for the vacuum. This I believe to be a better principle than that of using the same spring both in tension and compression at the same time; for there are very few springs that will come back to the same point when compressed as when extended. With this kind of spring it will not be necessary to grind them down to get the proper correction. They can be shortened or lengthened by moving the fixed support, and so adjusted very quickly and with great accuracy. As the spring is but a straight, square, tempered steel wire, it must be considerably less expensive than the present sort.

Among the various sources of error, there is one arising from change of temperature in the spring. This placing of the spring outside the piston-barrel ought to help that; or, at least, it will certainly be subject to a lower temperature outside than in. It seems to me that one of the best methods of avoiding all error would be to take on each card a pressure as well as an atmospheric line, and then adjust a scale to correspond to these two lines. I have devised an adjustable scale, to use in the place of all the scales now provided, which is applicable to every case. It is shown in Fig. 8.

Figure 9 shows a card, one-half the real size, taken from a 6 x 12 engine at 330 revolutions. It was taken under very favorable circumstances, low steam being brought a long distance through uncovered pipes.

Figure 10 shows a card taken from one of my brother's steam hammers at the Sanderson Bros. Steel Works at Syracuse. The hammer was at the time making from 300 to 325 strokes per minute.

I have become so accustomed to devising what I suppose to be new, and then learning that it is old, that I shall not be surprised if told before I leave this room that most, if not all, of this is old; but I can only say that I suppose it to be new and believe it to be good.

APPENDIX.

In designing the indicator the problem of converting the motion of the piston into that of the pencil-arm, so that the latter should be a true representative of the former, early presented itself. The problem may be stated as follows: Given two points, one moving on a circle and the other on a straight line; required a mechanical connection between them, preferably of link work, which shall cause them to simultaneously describe equal increments of their paths.

It is evident that, if in Fig. 11, *a b* represent a pitch-line of a rack attached to the piston, and the arc *a c* the pitch-line of a pinion attached to the pencil-arm, the motion would be a proper and correct one. If the line *a b* equal the arc *a c*, and the rack be moved in the direction of its length till *b* comes at *a*, *c* will then evidently also come at *a*. From this we can derive a clear idea of the nature of the problem. Some means must be found that shall cause *c* and *b* to arrive simultaneously at *a*. In other words, we must rectify the arc *a c* upon the line *a b*. The problem was solved by an application of Rankine's rule for the rectification of an arc. The rule is as follows: Let *a c*, Fig. 11, be the arc, and *a b* its tangent at *a*. Draw the chord and bisect it. Make *a d* equal to one-half of the chord. With *d* as a centre and *c d* as a radius draw an arc intersecting *a b* at *b*; then *a b* is the straight line equal to the arc *a c*. If now *a b* be, as before, the line of motion of the piston-rod, and the arc *a c* of a point in the pencil-arm, and a projection be made on the piston-rod which shall include the point *d*, and a link be made which shall connect *d* with *c*, evidently, if the piston-rod be moved, the points *b* and *c* must reach *a* simultaneously, since they are at equal distances from *d*. Now, it is evidently unnecessary that the piston move on the line *a b* and have a projection including *d*, but it may pass through *d*, provided only its direction of motion be parallel to *a b*. We have thus secured a practically perfect connection for a motion equal to *a b*, but it will not be perfect for motions of smaller extent, since, strictly, each new arc would require a new centre and a new length of link. It, therefore, only remained to determine the error introduced between *a* and *c*, and see if its extent was too great to be allowed.

Figure 5 represents the motion, the figure being a photographic reduction from a carefully made drawing of twice the scale. Different positions of the piston are shown. For each of these the corresponding position of the pencil-arm was determined. The arcs

included between these points and the point a were then rectified upon the straight line $a\ b$ by Rankine's rule, a new centre being properly taken for each arc. Upon the left of $a\ b$ are shown the actual positions of the arm as determined by the mechanism, and upon the right the proper positions as spaced off by the dividers. It will be observed that the entire arc swept through by the pencil is not taken for exact rectification, but the pencil is allowed to go beyond the true point of rectification till the errors at the ends of the motion about equal those at the middle. By this means the maximum error was considerably reduced. A method was devised of still further reducing this error, by making the motion exact at three points instead of two, but the above slight error was considered a smaller evil than the weight of the additional moving pieces involved. The error of the motion shown in Fig. 5 has been determined mathematically, and its maximum value at the end of the pencil-arm found to be $\frac{1}{125}$th inch.

THE HUMBOLDT-POCAHONTAS VEIN, ROSITA, COLORADO.

BY R. NEILSON CLARK, ROSITA, COLORADO.

THE discovery of a thin pay streak, yielding carbonates of copper, native silver, and perhaps chloride of silver, was made on the 9th of April, 1874, within the trachytic belt which forms part of the Sierra Mojada. The discoverer named it the Humboldt Mine.

On the 2d of May, 1874, another discovery was made, in a northwest direction from the Humboldt, at about five hundred feet distant, this was named the Pocahontas Mine. After some exploration these two discoveries were proven to be on the same vein. To the southeast of the Humboldt a claim had already been taken up, called the Virginia; and to the northwest of the Pocahontas, one called the Southeast Leviathan. On these last two this same vein has been found and worked.

This paper will therefore treat of the Humboldt-Pocahontas vein, as worked on these four claims, to wit: beginning at the northwest, the Southeast Leviathan, the Pocahontas, the Humboldt, and the Virginia claims. Since the spring of 1874, there has been sunk on this vein a total of twenty-seven hundred and fourteen feet of

shaftings, forty-six hundred feet of levels have been driven, about six thousand square fathoms have been mined, and over one-half million dollars of silver have been shipped. The vein has been proven for four thousand feet of workings.

Since so much notoriety has been given to the "Geognosy, Geography, etc., of the Silver Region of the Sierra Mojada," by the very popular pamphlet, published in 1876 by the mineral surveyor of the district, I cannot approach these important subjects without great delicacy, yet perhaps a few words would not be amiss.

The Arkansas River after debouching from its grand cañon flows about south 70° east through the city of Pueblo. Its cañon at Cañon City forms the northwest limit of the Sierra Mojada (the West Mountains), commonly but erroneously called the Greenhorn Range, the name of its most important peak being "Greenhorn."

This range has an approximate trend of south 20° east. In the wedge-shaped piece of country thus formed between the south bank of the river and the diverging range lies the Cañon City coal field. Much discussion has taken place in regard to the geological age of this coal. Some assert it to be Upper Cretaceous, others Lower Tertiary; for my own part, I have to record, after years of daily work in it, that I have never seen a single Cretaceous fossil above the clay bed, which is always present under this coal basin (which dips from its outside edge to a common centre like a bowl), nor one single Tertiary fossil below it. I am of the opinion that want of care in ascertaining the exact stratigraphical position of the fossils has led to all the voluminous discussion of the subject; but, be this as it may, I doubt if any paleontologist would deny that the upper sandstone rocks of this coal formation are Tertiary. These upper rocks are tilted up, as the coal beds under them are, against the range. We can judge therefore that this range was elevated after the deposition of these coal beds, probably during the Tertiary period, and it may be during one of its earlier epochs.

That we have a right to designate two apparently similar formations as being of the same age, when separated by nineteen hundred miles, simply by the similarity of their minerals and rocks, seems to me to be a conclusion calling for grave consideration; there is, however, every similarity between the red granitic rocks forming the Sierra Mojada and the rocks of the Laurentian period of Northeastern New York, even to the frequent occurrence of beds of magnetic iron. On the western slope of this range, forming part and parcel of it, intermingling its broken edges with the granite, lies the

trachytic formation in which is found the Humboldt-Pocahontas vein. It seems probable therefore that it belongs to the Tertiary.

This belt of trachyte has, so far as I know, never been measured; it has been stated that it is fifteen miles long by three wide. I think this is an exaggeration. It varies as all such belts do, many other rocks being found in it—andesite and dialogite are not uncommon; and to the northwest of the Humboldt-Pocahontas vein brecciated trachyte occurs. In one of the peculiar-shaped hills of this material is found the rich segregation, known as the Maine Mine, owned by Edward Bassick, Esq., which is now yielding about $8000 a month in gold and silver. This deposit is about fourteen feet by twenty-one feet. The shaft takes out all the mineral matter. Its minerals are galena, zinc-blende, with some tellurides, and free gold.

To the southeast pitchstone is found; to the southwest ash-beds, containing those peculiar vegetable-shaped nodules of quartzitic rock, also beds valuable for building-stone, being easily quarried and dressed, and after even limited exposure becoming hard.

From any of its higher peaks one can roughly trace the limits of the belt; its peaks, whether high or not, are easily distinguished by their round heads, smooth sides, and verdure, from the broken hills and ranges of the surrounding granitoid formation. In the basin formed by the higher peaks lies the Humboldt-Pocahontas vein. Its trend is about north 50° west, its pitch is to the southwest, therefore away from the range at an angle varying from eighteen to thirty degrees.

I shall now describe each one of the four claims in detail; the reports being closed on work performed January 1st, 1878.

The Southeast Leviathan.—This company has worked to a depth of one hundred feet; its drifts and shafts show the usual characteristics of the vein. It appears that this company like many others has not the means to fight against the water that is gaining on them. The whim hoist is of good character, but it does not give them the advantages which even the cheapest kind of steam hoisting and pumping would do.

The total amount of shafting is 171 feet; of drifting, 190 feet; of tons shipped, perhaps, 50.

The currency value of ore shipped is $4300, which gives per ton the value of $86.

Its boundaries with those of the Pocahontas overlap slightly; this causes apparent confusion on the accompanying sketch (see page 26).

The Leviathan vein has also been worked on this property.

The Pocahontas Mine.—The original discovery shaft being found to be within the Humboldt property, a "miners' meeting" was held, and the question having been left to arbitration the overlapping two hundred and thirty feet was divided; each one of these two claims became, therefore, 1385 feet long. Two shafts were started and worked by whims to a depth of about 175 feet, where, after passing through the first level, they met the second level, which was connected to the outside by a tunnel two hundred and eighty feet long. These two original shafts have, since the making of this connection, been used only as air shafts, thus obtaining a supply of air that has always needed checking. A main vertical shaft was then started about two hundred and thirty feet to the west and south of the vein, with the intention of cutting it at a depth of about eight hundred feet. At a depth of one hundred and forty-nine feet an adit was driven, connecting this shaft with the three-hundred-foot level. A winze passing downward on the vein from the two-hundred to the three-hundred-foot level, completed the circulation of air to the lowest level. The shaft is now seventy-one feet deeper. It is intended to drive adits from it to the vein so as to have a system of levels one hundred feet apart. Over the main shaft stands a handsome friction hoisting-engine and boiler of thirty horse power; and a small air compressor, worked by common connection with the hoisting engine, produces power for a Knowles pump, which, placed at a spring near by, supplies water for the boiler. The mine makes but little water and all of that in the main shaft. All of this work, together with the chutes, screens, etc., are very neat; they were all made by Frazier and Chalmers of Chicago. The main shaft is sixteen feet long by eight feet; it is divided into three compartments, two, six by eight, for hoisting; one, four by eight, for manway; it is strongly timbered.

The total amount of shafting is 690 feet; of drifting is 1834 feet; of stoping is 2300 square fathoms; of tons shipped is 2559.

The currency value of ore shipped is $317,477.51, which gives per ton the value of $124, and per square fathom the value of $138.

This yield does not include the waste-ore dumps nor the broken rock in the stopes; the owners consider that these will become of great value when they have erected their own dressing and reduction works.

Mr. Allen A. Herr has kindly given me the following statement taken from the books of the company, to which I have added the last column.

Statement of Ore sold from the Pocahontas Mine at Rosita, Colorado, from September 29th, 1874, *to January 1st,* 1878.

Year.	Weight. Tons.	Lbs.	Total ounces.	Coin value.	Received from Mills.	Currency value.
1874,	67	169	9,754.56	$12,611.67	$8,540.23	$14,188.13
1875,	639	1553	85,434.78	110,458 63	73.318.03	128,132.01
1876,	760	1788	75,117.05	97,118.83	61;716.12	106,830.71
1877,	1091	790	50,815.00	65,698.71	27,673 53	68,326.66
	2559	300	221,121.39	$285,887.84	$171,247.91	$317,477.51

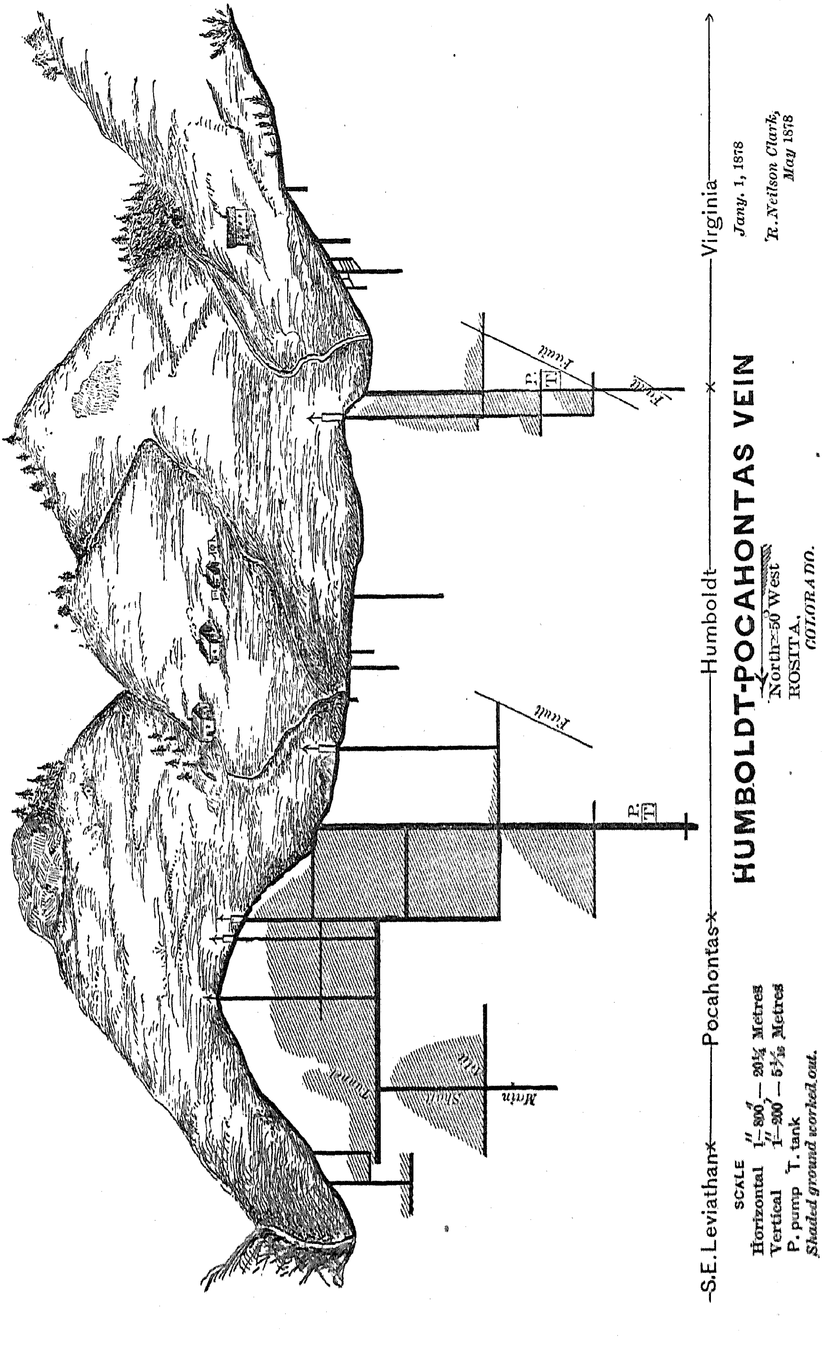
The yield in 1877, was principally from the waste-ore dumps, attention being given to sinking, etc.

The Humboldt Mine.—This claim was originally sold for about twenty-five dollars' worth of pork, flour, etc. When the shaft was fourteen feet deep, one-half sold for one hundred dollars; when one hundred feet deep, one-eighth sold for five thousand dollars; and when one hundred and seventy feet deep, one half sold for thirty-two thousand dollars cash. This last transaction took place in September, 1875. During the following month the Humboldt Silver Mining Company was organized; this company purchased the title of the Humboldt claim together with the South Humboldt and West Virginia claims, these claims being between it and the Virginia Mine. These purchases gave the company about nineteen hundred feet on the vein. All of the capital stock was at once issued to cover the purchase of the real estate; its working capital being but the few tons of ore on hand at time of purchase. It has never called for any assistance, either by selling stock, assessments, loans, or overdrafts against ore in transit. Its history is therefore one of the most remarkable on record.

After a little surface ore was taken out by drifting, a shaft thirty feet long, for double hoist and manway, was started on the vein. At every hundred feet levels have been turned. This shaft is now down four hundred and twenty feet. The engine hoist only permits of working to advantage to the three-hundred-foot level, but new machinery will soon be placed.

As shown in the sketch, the vein has been prospected from one end to the other of the property. Throughout, the vein has been found in shape with the exception of one place, as shown.

The total amount of shafting is 1343 feet; of drifting 1885 feet; of stoping 3100 square fathoms; of tons shipped 2105.


S.E. Leviathan
Pocahontas
Humboldt
Virginia
Main Shaft
Fault
Fault
Fault
P.
T.
HUMBOLDT-POCAHONTAS VEIN
North 50° West
ROSITA,
COLORADO.
Jany. 1, 1878
R. Neilson Clark,
May 1878
SCALE
Horizontal 1''–800' — 201/4 Metres
Vertical 1''–200' — 5 1/16 Metres
P. pump T. tank
Shaded ground worked out.

The currency value of ore shipped is $225,604.15, which gives per ton the value of $107, and per square fathom the value of $73.

The following statement is from the books of the company:

	Shipped. Tons.	Lbs.	Currency value.	Received from Mills.
1875, ...	99	1384	$43,749 52	$28,677.05
1876, ...	403	563	57,034 61	39,211.02
1877, ...	1477	1695	98,109.35	47,915.32
	1980	1642	$198,893.48	$115,803.39

Before the organization of the company the mine had yielded about 133½ tons, containing $26,710.67 currency value, for which it had received $16,341.67. The hoisting machine is a light twelve-horse-power engine, geared by cog-wheels to a wooden drum. At 350 feet deep a small Ferrell & Jones fly-wheel pump has been placed, with a one-inch steampipe, and one and a half water discharge; it throws nine hundred gallons of water per hour a vertical height of three hundred feet.

On the southeast end of the property a thin but rich parallel vein is also worked; it is thirty feet off on the foot-wall side. Many believe it to be part of the main vein. The advancers of this theory consider all intervening rock to be fissure matter.

The Virginia Mine.—Its original location was on the narrow streak just referred to; the main vein was soon discovered and worked. The main shaft, located on a rich chimney, 90 feet from the Humboldt line, was about 80 feet deep when the property was purchased by the Virginia Mining Company of Rosita, in April, 1876. This shaft has since been sunk to a depth of 355 feet.

The total amount of shafting is 510 feet; of drifting 700 feet; of stoping 465 square fathoms; of tons shipped 178¾ tons.

The currency value of ore shipped is $18,547.85, which gives per ton the value of $103, and per square fathom the value of $40.

The following statement is from the books of the company:

	Shipped. Tons.	Lbs.	Currency value.	Received from Mills.
1876, ...	55	1652	$8,494.21	$5,239.78
1877, ...	104	187	6,756.80	2,567.64
	159	1839	$15,251.01	$7,807.42

To this must be added for returns received previous to the organization of the company:

	Shipped Tons.	Shipped Lbs.	Currency value.	Received from Mills.
1875, ...	10	1770	$2,200.84	$1,413.78
1876, ...	8	. . .	1,096 00	600.00
Total, ...	178	1609	$18,547 85	$9,821.20

The company exhausted all its available funds while working in a fault found both in the level to the southeast and in the shaft; this fault is similar to the one noted as occurring in the highest southeast level of the Humboldt; and, at this date, May 1st, in the next lowest level. The last fifteen feet of sinking in the main shaft showed the vein in its normal condition. The wall had been pitching off at 62° from the perpendicular, but it suddenly changed to 30°, this last figure being about the average of this shaft pitch before any irregularities were noticed. At the same place the pay streak came in, with its accustomed gouge of soft clay.

The machinery of the Virginia deserves passing notice, on account of its great adaptability for working prospecting shafts, especially under the disadvantages of high freights and dangerous wagon-roads. The engine and boiler were made by Williamson Bros., of Philadelphia. The boiler is a simple strongly built locomotive boiler, twelve feet long by forty inches in diameter; its steam-dome is large, its fire-box five feet long. It supplies the engine and three pumps.

The friction-geared engine is one of their own design; it is self-contained, the cylinder, etc., being cast on the bed-plate, which is five feet nine inches by five feet. The drum, friction-gear, and levers work inside of this. The cylinder is ten inches by ten inches, the drum thirty inches diameter. The boiler feed-pump is one of Ferrell & Jones's fly-wheel pumps. The total weight of engine, boiler, pump, and fittings was under one car-load.

The engine having been set up at the shops, arrived late one evening, heavy timbers having been prepared to receive it. The next day the engine was unloaded, the third day it started, and without any trouble did duty. The only patent that I know of on the engine is an adjunct steam valve; the patentees call it the governor valve; it is connected with the lever which throws the friction-gear in connection, so that when the lever is thrown over additional steam is gradually admitted. At starting, therefore, the motion is slow, and at the same time the friction-gear does not grind; the usual

heavy cutting of the V faces is thus avoided. At thirty pounds boiler-pressure the engine readily hoists twenty barrels of water an hour, including filling and dumping, from a depth of three hundred and fifty feet. It is easily carried on a single wagon, as it only weighs a little over two tons.

The mine pumps were furnished by the Enterprise Hydraulic Works of Philadelphia. One of them, a number five, Ferrell & Jones fly-wheel pump, was set one hundred and eighty-four feet deep. Its steam-cylinder is ten inches by ten inches, its water-cylinder four inches in diameter, and its weight about seventeen hundred pounds. It received steam through a one-and-a-half inch pipe, and discharged water through a two-inch pipe. It pumped from a tank into which all the water from above was collected, and it had an easy capacity of twenty-five hundred gallons an hour at a vertical throw of one hundred and sixty feet.

Below it was placed a Nye vacuum pump, which discharged into the above tank. It was lowered into the sump foot by foot as required, being easily handled by two men. It threw about seven hundred gallons an hour up a vertical height of about seventy feet, using an inch steam-pipe. One of its great advantages is, that if it is necessary, the mine water can be permitted to thoroughly flood it, for it works, if anything, better when it is submerged.

The Ferrell & Jones pump was assisted by Fould's water elevator, a simple arrangement, that permits the exhaust pipe to pass into the suction pipe, thus utilizing the exhaust steam.

This plant was not called on for any extreme work, but it is worthy of notice that all the machinery was worked with a boiler pressure of about fifty pounds, with the consumption of about one and a quarter cords of wood each twenty-four hours; that it was easily handled and did not get out of repair.

All work on the vein, following the example of the Humboldt, has been done on the simple long-wall system. This has saved the frequent driving of winzes, and gives, as usual, more perfect ventilation, which is always under control. Driving drifts usually occasions the greatest trouble to all managers, but by this system pure air is always kept forward; the miners, after carrying say ten to twenty yards, return and take the first stope down, the stulls and lagging are at once placed and the lagging covered by the refuse of a higher stope. The return thus formed is connected with the up-cast, and the circuit is complete. It is easy to carry air by this means five

hundred feet; and, at the same time, the stopes and chutes are prepared for work.

The higher-graded ores have usually been shipped to Northern Colorado, the lower-graded have been treated at Mallette Works at this place, or at the Pennsylvania Reduction Works. This last company was organized by the stockholders of the Humboldt and Virginia companies, in order that they could receive some return from their low-graded ores.

The process is chlorination and amalgamation, and it yields better results than was expected. The mill treats about one hundred and forty tons, its full capacity, per month. Great trouble was experienced at the start in roasting, the antimony and copper in the ores interfering with the chlorination in the Brückner cylinder. The introduction of a steam jet at the back of the fire-box has been of great advantage, and since this was adopted I believe the mill has made average savings, its record on cost per ton being very good. It is found that more silver is taken up than is chlorinated in the cylinders; especially is this so when time is given to the grinding in the pans before the introduction of the mercury.

The amalgam varies very much in fineness. By giving time and using very low heat at the start on the retort, it is found that the more cupreous sponge is found to be on top. This is easily separated by a chisel, and is shipped in the form of copper-silver bricks to the Boston & Colorado Works, at Black Hawk, Colorado. The finer bricks are shipped to New York.

RÉSUMÉ.

	Shafting.	Drifting.	Adits and tunnels.	Stoped.	Shipped.	Currency value.	Mill returns.	Average per sq. fathom.	Average per ton.
	Feet.	Feet.	Feet.	Sq. Fath.	Tons.				
Southeast Leviathan....	171	190	....	....	50	$4,300.00	$2,100.00	....	$86
Pocahontas..........	690	1,834	460	2,300	2,559	317,477.51	171,247.91	$138	124
Humboldt.................	1,343	1,885	....	3,100	2,105	225,604.15	132,145.06	73	107
Virginia	510	700	140	465	179	18,547.85	9,821.20	40	103
	2,714	4,609	600	5,865	4,893	$565,929.51	$315,314.17		

The vein, with an average trend of north 50° west, conforms to the part of the range in which it occurs. Its hanging wall is remarkably regular and smooth; the foot-wall is less regular, and often swells from the hanging wall. Many insist that the vein is at least twenty-five feet thick; they assume that a parallel streak occurring on the foot-wall side is part of the vein, and that all the rock between is fissure matter. I question the correctness of this view, for in most places both walls of this one pay streak are identical in all respects, except perhaps smoothness and hardness. The gangue is a soft clay, easily mined as a gouge, occurring usually towards the foot-wall; it is undoubtedly decomposed trachyte, usually showing the characteristic color, etc., of that portion of the wall against which it lies. The pay streak usually lies against the hanging wall, often separated from it by a clay selvage. It is always accompanied with heavy spar (remarkably free from rhomb-spar), and galena is also found. Generically the ore is a barytic-tetrahedrite; copper and iron pyrites are common, together with stephanite and the like. The more valuable specimens are the antimonial compounds of silver and rarely a pseudomorph of tetrahedrite, in which it assumes the form of heavy spar. The ore has averaged about one hundred and twelve dollars a ton, and a small percentage of copper throughout all the workings.

I have drawn in the sketch (page 26) the hills that lie immediately back of the vein with sufficient accuracy to show their dome-like shape. Examination does not give the slightest reason to suppose that each one was of separate eruptive action; on the contrary the slopes of most of these hills run into each other as smoothly as the wind-hills on a sand-beach. They have the appearance of owing their present shape to erosion. This idea is strengthened by the finding in the two valleys that pass through the vein, and under which the workings have penetrated, an entirely different character of rock. It is only under these valleys and in this extraneous rock that the vein has shown even the slightest fault.

The locations of these faults are shown on the sketch; the one is on the Humboldt, the other on the Virginia property. There is no appearance of any especial disturbance at either place; but, on the contrary, the fissure appears simply to have attempted, while it was opening, to keep its usually good reputation for straightforwardness and a single course, but utterly failed in this, to it, strange rock.

The vein-matter is always in these places of the very poorest character and of no value. This rock might be called a melaphyr;

its coarseness prevents my giving it any name that would be recognizable. Chlorite occurs in it in large masses, and a dark-red impure dialogite (carbonate of manganese) is one of its constituents. That these two valleys have been formed where this heterogeneous rock occurs is positively proven, and since the vein is also out of condition at these places, it might be well for the miners of this district to beware of working in the flats except at great depth.

At those places where the vein has been worked under high points, it has always been found to be strong and reliable; for example, the main workings of the Pocahontas and Humboldt, and the furthest southeast shaft of the Virginia, when ore within fifteen feet of the surface yielded at the mill one hundred and fifty ounces per ton.

Constant examination of the vein-matter has led me to the following conclusions in regard to its formation. It would appear that the fissure was at first very narrow; the action of water gradually filled it with a clay formed from the decomposition of the walls. This clay still remains in many places as a selvage against the hanging wall, and shows by the markings on it, usually more or less perpendicular, that subsequent movements of the walls took place.

Another gaping followed, and the barytic ores were deposited. Every specimen of rock that I have been able to find, away from surface action and from any known vein, as well as specimens taken from near veins, has contained heavy spar and some one or the other of the copper or lead minerals, as seen under the microscope. It seems possible, therefore, that this vein-matter is but a concentration of these small particles of ore. The possibility of barytic spar being dissolved by and deposited from mineral waters, I think has been proven by Sanberger, and to this may be added that in testing the water from the springs in this neighborhood, I have always detected baryta, usually, I think, as a carbonate. The other minerals can be so moved without doubt.

Segregation and crystallization then followed, the streak usually remaining against the hanging wall, and the space thus formed was filled by the clay eroded from the walls. This clay is the gangue of the vein. Quartz is never present, excepting in minute crystals. It is possible that subsequent openings took place, but they were probably confined to limited portions of the vein.

The presence of boulders in the vein is frequent; they vary from one inch to six inches through. One taken from the Virginia shaft at a depth of three hundred and five feet is remarkable in showing the different characters of the rocks described above when speak-

ing of the fault, not, however, intermingled, as one would expect, but in layers and masses. This boulder is full six inches in diameter, and has the coarseness of texture, etc., of the foot-wall rock against which it was formed, a rule that all I have observed follow.

ROSITA, COLORADO, May 10th, 1878.

THE MODE OF COMBUSTION IN THE BLAST-FURNACE HEARTH.

BY PROF. JOHN A. CHURCH, E. M., COLUMBUS, OHIO.

It is a well-known fact that under similar conditions a ton of pig iron can be made from any ore with less fuel when charcoal is used than when coke or anthracite is employed for heating. The cause of this superiority is entirely undiscovered, and no explanation has yet been offered which has won even temporary acceptance. Such as they are the hypotheses heretofore presented are of two general kinds, and they come from those two divisions of the working force which are so often antagonistic in all professions, the scientific and the practical men.

The theory of the scientific men is drawn from the chemical reactions which are known to go on in the furnace, and it is well expressed in a recent paper by Prof. Akerman, of the Stockholm (Sweden) School of Mines. It makes the superiority of charcoal as a fuel in the blast furnace to depend upon its quick and thorough reduction of carbonic acid. This property belongs to charcoal in virtue of its porous structure. Its cells have a diameter of only one twenty-four hundredth of an inch, and one grain of charcoal has been computed to have a surface of two and four-tenths square feet. According to the theory in question, carbonic acid is formed by the first impact of the air upon the fuel; but in charcoal furnaces the acid is immediately reduced to oxide by taking the necessary amount of carbon into combination, and the gas reaches its highest reducing power even in the near vicinity of the tuyeres. Coke, being harder, does not produce so strong a reducing gas in that part of the furnace. Carbonic acid is formed as in the case of charcoal, and remains longer before its reduction to oxide is completed. Anthracite is the

most dense fuel known, and resists the attack of the carbonic acid longer than coke. These facts are pointed to by Prof. Akerman and other scientific men, not so much as a full explanation of the higher consumption of the two dense fuels in comparison to the porous one, but as the probable source from which such an explanation is to be drawn. Admitting the instantaneous reduction of carbonic acid before the tuyeres, it is still difficult to understand how this can affect the consumption of coal when all the researches show that the ore is just as completely reduced twenty feet or more above the tuyeres as it is at any point in the furnace. Up to the moment of fusion it contains a small amount of oxygen, estimated by Bell at one per cent., which is removed in the act of fusion by the deposited carbon with which it is in intimate contact; but with that exception reduction is completed in the upper half of the stack.

The practical men in the profession seem to be mostly unanimous in thinking that the high consumption of dense fuels has some dependence upon their slower rate of combustion. It is generally admitted that anthracite burns less rapidly than coke or charcoal, and as a certain weight of carbon must be oxidized in each second to maintain the heat required, they say that there must be more of the dense fuel present to make up for the slower combustion of each piece. The relation of slow combustion to increased fuel consumption has not been very clearly stated, perhaps because the fallacy of the logic that connects them is too plain. Slower combustion indicates the necessity, not of more fuel, but of larger hearths, which will contain more pieces, and present more surface of carbon to the air. If the ignition of a fixed quantity of carbon in one second is all that is required, it will not be necessary to increase the fuel in proportion to the ore, but merely to present the physical conditions which are necessary for burning as much carbon of the dense fuel in one second as of the porous fuel. In that case the *blast* would be constant, but it is well known that anthracite furnaces require much more wind than coke or charcoal stacks.

But the scientific explanation is equally fallacious. It is based upon experiments which cannot recommend themselves as a foundation for accurate deduction. I think no one acquainted with blast furnaces can readily acknowledge that trustworthy gas analyses can be obtained from the centre of the crucible or the point where the heat of the furnace is highest. The logic of this theory is also the worst possible.

Combustion gives the most heat and the highest temperature when

the product is carbonic acid. Reduction of this acid to oxide is a cooling process, being always accompanied by absorption of heat. Now if the dense fuel allows the least of this reduction in the crucible, it ought to secure the highest heat there. That is to say, the crucible of the anthracite furnace should be hotter than the crucible of a charcoal furnace, using an equal weight of fuel and with the same ores. But that proposition we know is untrue. Anthracite furnaces use much more coal than charcoal stacks, even when working under conditions which are nearly similar. In the paper on the Velocity of Blast Furnace Gas, presented to this Institute at the Dover Meeting in May, 1875 (*Transactions*, vol. iv. p. 119), I showed the following comparative results of the two furnaces working on Lake Superior ores, one using charcoal and the other anthracite.

	Elk Rapids. Charcoal.	Fletcher. Anthracite.
Ore, per ton pig; tons,	1.613	1.730
Limestone, "	0.145	0.617
Charcoal, "	0 884	....
Anthracite, "	....	1.427
Average yield of ore,	62.0	57.8
Average grade of pig,	1.05	2.11

The Fletcher anthracite furnace, therefore, used 0.117 ton of ore, 0.472 ton of limestone, and 0.543 ton of coal more than the charcoal furnace. The ore used in the anthracite furnace was not so rich by 4.2 per cent. as that of its rival; but it also produced a pig metal which was lower by 1.06 numbers. Furthermore it did not work on anthracite alone, 0.371 ton of its fuel being coke. This addition of coke and reduction in the grade of the product may be assumed, without close calculation, to offset the better quality of ore in the Elk Rapids furnaces so far as the expense of heat *in fusion* alone is concerned. We then have 0.472 ton of limestone and 0.543 ton of fuel more in the anthracite than in the charcoal furnace; and we may confine our calculation to the difference in the burden of limestone. The 0.472 ton of flux contains 56.64 lbs. of carbon, and the fuel will lose a corresponding amount. The heat absorbed in this process will require 87 pounds of coal, if we assume that the average heat of production is one-third the maximum of pure carbon. To expel the carbonic acid 172 lbs. of coal will be required under the same supposition. These three quantities sum up 316 lbs., or 0.141 ton, and we may look upon the remainder of the difference (0.543—0.141) 0.402 ton, or 900 lbs. of anthracite

per ton of pig, as probably representing the nearly inherent difference between the two fuels. There can be little doubt that this difference would be enormously increased if the Elk Rapids furnace with its 1905 cubic feet capacity were to be fed with anthracite, and driven to make 29.75 tons of pig in twenty-four hours. Or looking at the subject from another point of view, there can be little doubt that the Fletcher furnace, with its 4193 cubic feet of capacity, would quickly chill throughout, if 900 lbs. of anthracite per ton of pig were subtracted from its charge, and it were still driven to produce 25.27 tons of metal as a constant daily average. It would not have heat enough for its work either in the hearth or anywhere else.

Returning to the two theories which have been spoken of it will be observed that both of them recognize the fact, that the difference between the fuels is confined to their action in the *hearth* of the furnace. Stated in a few words, the explanation of the chemists is, that the charcoal furnace is better because it has a strong reducing gas in the hearth; and the explanation of the practical men is, that the charcoal furnace is better because it has higher temperature in the same region, both propositions being based upon the assumption of equality in weight of fuel, rate of driving, and all other conditions.

In this day when theory rises upon theory from every point of scientific view, it is with some regret that I feel compelled to reject the explanations mentioned above, and offer one that is every way new. But I think my hearers will agree that the occasion demands it. It is assumed that no theory can be conclusive which does not satisfy the following conditions:

That for a given weight of fuel the well of the charcoal furnace is hotter than that of the coke or anthracite furnace.

That more fuel reaches the lower part of an anthracite furnace than of a coke or charcoal furnace.

The explanation offered as sufficient to satisfy these conditions, leans more to the time theory of practical furnacemen than to the carbonic acid reduction theory of the chemists. It depends upon the different igniting powers of the three solid fuels, and it draws the attention away from the fuel in order to fix it upon the behavior of the *air*. I hold that—

The highest carbon duty is given by the fuel which withdraws the most oxygen from the blast in a given space of time.

In other words, the carbon duty of a fuel is proportional to its power of combustion in extremely dilute oxygen.

Let us consider what takes place when air is blown forcibly into a mass of incandescent fuel. Air is a mixture of nitrogen and oxygen, and the proportion of the latter is such that it is eagerly absorbed by many substances. One of these is carbon when its temperature is elevated. This igniting power of carbon varies decidedly with the fuel. Charcoal is most easily ignited, a match or a few shavings being sufficient to set it in combustion. Anthracite requires a mass of live coals or the flame of a considerable quantity of wood for its ignition.

It is to be observed that the proportion of oxygen in the air is a very important point. Even with a fuel so easily ignited as a candle, the flame ceases when eight or ten per cent. of carbonic acid is present, and candles are extinguished every day in our mines from this cause. We cannot attribute this loss of igniting power to the lowering of temperature due to reduction of the carbonic acid by the flame, for even with eight or ten per cent. of the impurity the oxidizing power of the air is still too marked to permit the supposition that such reduction can go on. It is more probable that the carbonic acid acts as an inert substance replacing the same quantity of oxygen. The action is probably one of dilution merely, and we may correctly summarize the facts by saying, that when the oxidizing power of the air has been reduced by removing ten per cent. of its oxygen, a candle will no longer give a flame though its wick continues to glow.

In the blast-furnace hearth, where the heat is intense and the combustible is already incandescent, fuel of any kind is in a condition to take up the atmospheric oxygen quickly, so long as its proportion to the nitrogen is maintained within a certain limit. But if the explanation here advanced is a true one, that limit must be reached within a short distance of the tuyeres. Even a foot or two from the nozzle the gas can no longer be air, but must be a much weaker dilution of oxygen than it was when it entered the hearth. That change in the composition of the blast completely alters the conditions of combustion for the fuel through which it passes subsequently.

Almost any fuel would take up oxygen eagerly from a gas that contained twenty-three per cent. of it, but when the proportion is reduced to ten per cent., or to some other limit not yet fixed, the fuel that combines with oxygen most readily begins to assert its superiority.

It withdraws the greatest proportion of the oxygen from the air

while the blast is still in the neighborhood of the tuyeres, and this is the reason why a given weight of charcoal makes a hotter crucible than the same weight of carbon in any other form. (This is the first of the requirements given above.) We are not to suppose that charcoal burns as well in an atmosphere containing only ten per cent. of oxygen, as in one that has more than twice that proportion. But it seems to burn with much more avidity than coke or anthracite under these conditions, and it is to this power of burning in a weakly oxidizing atmosphere that charcoal owes its immense superiority over all other fuels.

Time is a necessary element in the problem, because when combined with velocity it represents a certain path or distance travelled by the air during the time its oxygen is burning the carbon. And that fuel is the best which allows the oxygen the shortest path before it is completely absorbed, because in that case the greatest portion of the path is included in the fuel below the zone of fusion, and more heat is produced below this zone.

Charcoal with its porous structure offers the greatest surface for oxidation, while its great bulk reduces the amount of mine, and accordingly of work to be done, in a zone of fusion of given capacity. These are the physical conditions which make it a better abstracter of oxygen from the blast than its competitors.

The state of things is very different in an anthracite furnace. That fuel combines readily enough with the blast so long as it is rich in oxygen, but when a certain limit is reached in the proportion of this element the combustion becomes very slow, and the last portions of oxygen are carried up into the boshes and consumed there, away from the zone of fusion. This oxygen which is thus carried too far represents a given quantity of carbon which failed to burn in the crucible, but which must be burned there in order to obtain the requisite temperature. To burn it an increased amount of air must be blown in, and when its oxygen has been reduced from the normal proportion we have, it is true, obtained the necessary heat in the crucible, but only by loading the furnace with a quantity of unused oxygen which is gradually consumed above the zone of fusion. To provide for this combustion an increased quantity of fuel has to be charged.

It might be possible to follow this oxygen through the upper zones of the stack and trace its effect upon the operations there. But it is sufficient now to say that the first result would be the addition of heat to the materials just before they entered the zone of

fusion, and that this advantage might be entirely neutralized if the gases were left hot enough to increase the amount of carbonic acid reduced. In fact the latter action might more than neutralize the gain obtained by the regenerative action of the materials, and compel the addition of fuel to compensate for the loss. The fact that anthracite furnaces require so much more carbon than charcoal stacks indicates the probability of some action of this kind.

On entering the furnace the blast is subjected to two forces, one of horizontal propulsion from the nozzle, and one of vertical propulsion, due to the fact that its point of discharge is placed above its point of entrance. Its path, the resultant of these two forces, is a curve. The combustion or abstraction of its oxygen takes place along this curved path, and in wide hearths filled with anthracite it is possible that the centre or axis of the crucible may contain material that is merely incandescent, the air that reaches it not being rich enough to maintain the temperature of fusion. In charcoal furnaces this central portion would be hotter from the greater ability of the fuel to combine with oxygen when in a state of dilution.

The two examples drawn from anthracite and charcoal practice for the purpose of ascertaining the approximate superiority of charcoal when working under conditions nearly similar, do not fairly represent the possibilities of hard coal. Mr. Witherbee has given the Institute an instructive example of the economy which can be obtained from anthracite under conditions which are not at all favorable to economy. With an ore yielding about fifty-three per cent. of pig metal, and requiring 0.714 ton of limestone, he has reduced the item of fuel to 1.393 ton of anthracite, and recently, I believe, to about 1.2 ton. Mr. Firmstone has obtained just as good results at Glendon. Under the circumstances the practice in these two cases must be considered as good as that of the charcoal stacks, and a consideration of the methods by which this result has been obtained shows that they all point to one thing,—increase of oxygen absorption in the hearth.

The improvement has been threefold: increase of height and capacity of the stack; increase of heat in the blast; and increase of capacity in the hearth.

The first two of these improvements raise the temperature of the two elements of combustion and thus assist their combination. The third lessens the velocity of the blast in the hearth, and thus increases the time of contact.

The present efficient means of heating the blast may enable the

hard-coal furnaces to approach still nearer their rivals, especially as the charcoal men appear to be pretty near the end of their improvements. When a furnace produces a ton of pig metal by the combustion of 17.68 cwts. of a fuel that has been proved to contain from 25 to 35 per cent. of volatile matter, leaving say 13½ cwts. of carbon, the minimum cannot be far off even with ores that contain 62 per cent. of iron and only fusible silicates for gangue.

When we consider the state of facts as they exist in the working of a furnace, we shall see that they support the views here advanced. Of the three solid materials charged into a furnace, two are fusible when brought in combination, and possibly they are so when subjected alone to the high heat of the hearth. They are the ore and the flux. The other, the fuel, is infusible. In its descent the ore becomes altered into iron sponge, which is probably not fusible at any point in the furnace, except at that zone where the freshly formed carbonic oxide from the hearth meets the highly heated sponge. When the gangue consists of fusible silicates it is possible they may be melted out of the sponge above the zone of fusion, and thus the effectiveness of that zone be increased. But at all events at that point the iron itself melts, and all the fluid materials, running to the bottom, collect in the crucible, leaving nothing to fill the space above the bath and below the zone of fusion but the fuel. That cannot be melted, and accumulates in the well until it is removed by combustion. It is perfectly evident that the height of this mass of fuel in a given furnace will depend upon the amount of oxygen burned in the well per second, the proportion of mine to the carbon in the fuel, and the relative fusibility of the gangue in the ore. Other things being equal an infusible gangue-like quartz will give a lower column of fuel in the well than a fusible one like any of the silicates; for the reason that it may remain unmelted by the accumulation of heat in its descent through the furnace, and require the aid of freshly formed carbonic oxide as the iron sponge does. The hot gas from the hearth will therefore not be able (unless increased in quantity) to produce fusion over so wide an area, when part of its heat goes to melt the gangue, as when it is free from that duty and has nothing but the iron sponge to melt.

There is, therefore, a double disadvantage in these infusible gangues. They take more heat to melt them, and consequently add to the fuel required, and they also reduce the column of fuel in the well, which lessens the contact of the blast with the fuel. If it is desired to restore the zone of fusion to the same height and area

that it would have with a more fusible gangue, more fuel must be added.

Precisely the same result is produced by the use of a dense fuel, incapable of rapid combustion except in presence of a highly oxygenated blast. Less oxygen is consumed for a given height of the fuel column, less heat of course produced, and the area over which fusion can be maintained is again reduced, and the line of fusion falls down the sloping boshes until a proper balance is obtained between area and height.

The old notion of fusion "before the tuyeres," or anywhere in their neighborhood, is not to be entertained. Mr. John F. Bennett, in his admirable article on the "Construction of Blast Furnaces," published in the *Metallurgical Review*, for March, 1878, seems to be much nearer the truth in placing the zone of fusion about twelve feet from the sole, and eight feet or thereabouts above the tuyeres. (These dimensions are only approximate, and inferred from the drawings published with the article.) We must regard the blast furnace as a fire fed with fuel and ore by means of a vertical tube raised over the fire-place, and in which the fuel finally separates from the ore and flux by virtue of its infusibility, and collects in one place.

It is always difficult to reason from a scientific explanation to matters of practice. But in the arts such reasoning is necessary, and it is also the best test of theory. It is therefore for the purpose of inviting criticism that I venture to indicate some of the points which are deducible from the foregoing generalizations. Some of these are well known and incorporated into the common practice, and are given here merely as reasonable explanations of known facts.

First as to the manufacture of coke. Charcoal is acknowledged to be the best of fuels, anthracite the worst. Coke is an intermediate product, and with strange perversity we have always insisted upon making it as much like anthracite and as little like charcoal as possible. Coke according to the authorities must be hard, strong, ringing, and everything else that charcoal is not, in order to win a good name. Having a good natural fuel and a bad one, we are determined upon imitating the bad one whenever we make an artificial combustible. Coke furnaces usually stand half way between charcoal and anthracite in weight of fuel to the ton of pig metal. Perhaps by making a lighter article, avoiding high heats in coking and aiming to produce a porous, delicate, and easily ignited coke, it may be possible for the coke furnace to draw near her charcoal sister in

economy of fuel. In considering this innovation it would be well to remember that the best charcoal practice is now had with a fuel that is *very* lightly burnt. As much as twenty-five and thirty per cent. of gas is left in it, and some of the most successful managers take the half-burnt brands at full measure for coal, and encourage the burners to send them all in. It is under these conditions that the best charcoal practice is had in stacks of thirty-five to fifty feet height. The reason why a similar course has not been followed in coke practice, is a mistaken notion that such fuel is not strong enough to carry the weight of the ore. But this chimera of "weak" fuel should be passed by as belonging to the traditional period of furnace management. We are now able to put reason in the place of myth, and rise above notions that have not one recorded fact to sustain them. The notion that fuels like charcoal and lightly burned coke "crush" in the furnace, probably had its origin in the presence of deposited carbon in blown-out furnaces. We now know that to be a chemical product, and just as abundant in an anthracite furnace as in any other. This mistaken idea has maintained itself with singular perversity, considering the fact that solid blocks of charcoal are known to fill the crucible, and have always been taken from the fore-hearth when furnaces with open hearths were in vogue.

A change in our ideas upon this subject may have the greatest effect upon the distribution of metallurgical operations in this country and also upon our positive wealth. Coke is now carried from regions furnishing a strongly caking coal to districts at a great distance which are themselves underlaid by vast deposits of a dryer coal, which the prevalent ideas on this subject forbid the furnacemen to use. It makes a weaker coke, and therefore it will not "carry the burden." But this coal also yields a larger proportion of coke than its rival, as well as a coke of lighter texture. If new views on this important subject make it appear that this softer coke is better than the hard there will be a double economy, a saving in coke and a larger yield from the coal.

Such a change in the sentiment upon coke-burning would be of great value to States like Ohio and others in the central coal fields, where the larger part of the coal is condemned as not being "metallurgical." Most of the thicker seams, which are necessarily the first to be worked, lie under this ban, and if it can be removed the blast-furnace workers as well as the miners will be greatly benefited.

In advancing these views I am not ignorant of the fact that the

prevailing practice seems to controvert theory in the most conclusive manner, for the best practice is had with hard Connellsville coke, and the worst with soft coke from the interior coal basins. This is of course a very serious criticism upon the deduction I am making, but there are some considerations which lessen its effect.

Most of the fuels from which these lighter cokes are made are quite slaty. Washing is not always resorted to, and when it is used the necessary sizing is so much neglected that the light cokes are usually notorious for their heavy percentage of ash. The effects of a large quantity of ash in the coke are probably more serious than appears at first sight. Let us, for instance, consider the case of a coal with twenty per cent. of ash. As before pointed out the whole of this would reach the hearth without being fluxed. If the furnace burns fifty pounds of coke per minute, and consumes two pounds of carbon in the same time for reduction of the carbonic acid derived from the limestone, and if the coke originally contained ten per cent. of volatile matter, there would be only thirty-three pounds of carbon and ten pounds of ash consumed per minute. The ash would require 5500 pound-units of heat for its fusion, less the heat already stored up. This would probably require one pound of carbon, and the remainder or thirty-two pounds would be all that would be effective in the zone of combustion.

The use of such a fuel would inevitably introduce irregularity into the working. In the upper part of the stack there would be a strong lime charge, which would be neutralized only when the lime cinder, melted in the zone of fusion, ran down and mixed with the strongly silicious cinder produced from the ash in the zone of combustion. With the present tendency to a high lime mixture in almost all furnaces, it is easy to see that wherever a large proportion of the silica is kept away from the base at the moment of fusion, as it is when the fuel contains so much ash, the charge must be excessively basic until it passes through the vital point of the furnace for fuel consumption, the zone of fusion. There all the evils of a high lime charge are experienced, even when the mixture taken as a whole will produce an average cinder. Excessive heat is required in order to get the lime cinder through the zone of fusion and into the crucible, but that point once reached the high temperature is no longer needed, and perhaps only tends to reduce silica.

Next, as to the form of the furnace. Any horizontal section through the boshes will vary in area according to its height above the well, and the zone of fusion, therefore, will vary in area with

the height of the zone of combustion below it. With steep boshes the variation in area of the zone of fusion will be very great for slight changes in the height of the fuel column in the lower zone. It has already been pointed out that the height of the latter, and therefore the area of the former, are largely dependent upon the character of the ore and the composition of the charges, and it is quite conceivable that the effects of any variation in the materials charged may be concentrated upon these two zones. Some such explanation is needed to account for the very considerable perturbations in the working of the furnace, which sometimes follow additions to the burden that appear to be unimportant in themselves. On the other hand the less the boshes are inclined from the vertical the more restricted will be these changes in the area of fusion, and a more regular working should be expected. It is this fact which makes it an error to increase the greatest width of the furnace unless the parts below are so altered as to preserve the gentlest possible slope to the boshes.

Forms of hearth which increase the bulk of the fuel column in proportion to the area of fusion, will, of course, extract the most oxygen from the air in its passage through this column to the zone of fusion. In the first form of the Soho furnace, acording to Mr. Bennett, the area of fusion was sixty square feet, and the cubic capacity below it was, as near as I can estimate it, 467 cubic feet; or in the proportion of 1 to 7.8. In the second form of the furnace the area was 120 square feet, and the bulk of the zone of combustion apparently 1050 cubic feet; or as 1 to 8.75. This increase of capacity in the zone of combustion increased the stay of the blast in it by about twelve per cent. The consequent gain in oxygen absorption in this zone produced a saving of fuel which lessened the quantity of blast required, so that its retention in the zone of combustion was still further prolonged. Thus there were two sources of advantage, which reacted upon each other until the total improvement appears to have been about forty per cent., for Mr. Bennett says the blast-engine formerly made 33, and now 23 revolutions per minute. The saving in coke was fifteen bushels to the ton of pig.

The alterations of the Soho furnace were not confined to the hearth, but it can hardly be doubted that a large part of the improvement made in fuel economy and daily product must be credited to the provisions by which more perfect absorption of oxygen in the zone of combustion was secured.

*THE HEAT OF THE COMSTOCK MINES.**

BY PROF. JOHN A. CHURCH, E.M., COLUMBUS, OHIO.

ONE of the most striking phenomena connected with the mines on the Comstock lode is the extreme heat encountered in the lower levels. This heat is not due to the burning of candles, heat of the men, and decomposition of timbers, all intensified by bad ventilation, as was the case nearer the surface. It proceeds from the rock, which maintains constantly a temperature very much higher than the average of the atmosphere in Nevada.

The heat of these mines is a matter of more than usual interest, for they are the only hot ones now worked in the United States, and both in the present temperature encountered and in the increase which is to be expected as greater depths are reached, they appear to surpass any foreign mines of which we have a record.

Hot mines are known also in other countries, as in the tin and copper lodes of Wales, where one of the veins worked by the United Mines is known as *the* hot lode. It has springs which discharge water at a temperature of 116° Fahrenheit, the depth being 220 fathoms, or 1320 feet. The heat of the air in these workings is given at 100° to 113° Fahrenheit. The air is bad, and the heat in the drifts seems to be traceable to defective ventilation rather than to the real necessities of the case. Air is supplied through a small pipe, and is drawn from a place where the temperature is 95° Fahrenheit. Under such circumstances it is not surprising to read that in this hot mine the air is hotter than the rock, a state of things which I have never observed in the Comstock. Other mines have been reported to the British Coal Committee as having temperatures of 106° Fahrenheit and thereabouts, but the only positive comparisons that are available at this writing are the following, all from Cornish mines:

Mine.	Depth in feet.	Temperature. Air.	Rock.
Tresaveau, ...	1584	86°	83° to 85°
Consolidated Mines, ...	1500	87°	86°
" "	1722	94°	93°
" "	1764	94° to 96°	92½° to 93½°

* Read by permission of Lieut. Geo. M. Wheeler, Corps of Engineers, U. S. Army, in charge of U. S. Geographical and Geological Surveys, west of the 100th meridian.

These high temperatures appear to be partly due to the usual sources of heat in mines, and partly to chemical action in the rock, for the average depth in which the heat rises one degree Fahrenheit from the surface downward varies with the ground. It is given by Mr. W. S. Henwood as follows:

In granite,	51 feet.
In slate,	37.2 "
In cross veins,	40 8 "
In lodes,	40.2 "
In tin lodes,	40.8 "
In tin and copper lodes,	39 6 "
In copper lodes,	38.4 "

The copper-bearing lodes are therefore the hottest, and in Cornwall heated ground is thought to be a good indication of copper, just as hot ground is looked upon in the Comstock mines as a favorable sign of ore.

TEMPERATURE IN THE COMSTOCK MINES.

The rock in the lower levels of the Comstock mines appears to have a pretty uniform temperature of 130° Fahrenheit. This was the reading obtained for me on several occasions by Mr. Comstock, foreman of the Ophir mine, and about the same temperature was found by Mr. Perrin, foreman of the Chollar Potosi, Mr. Cosgrove, foreman of the Yellow Jacket (139½° F. and 136° F.), and by myself in the Crown Point and other mines.

These readings were obtained by placing a thermometer in a drill-hole immediately after the hole was finished, and leaving it there for periods varying from ten minutes to half an hour. Very little or no difference was discovered between holes which were drilled wet or dry, or if wet, between holes which were naturally wet, and those which were made so artificially. No doubt there must be some difference due to these varying conditions, but they are so slight as to be completely masked by the steady flow of heat from the rock during the exposure of the thermometer.

The holes in which the thermometers were placed were not sunk especially for this work of testing, but were the ordinary drill-holes made for the purpose of blasting the rock. They varied therefore from about ten inches to three feet in depth.

No variations in the height of the thermometer were found to be caused by this difference of depth, and this also is quite reasonable.

Mining on the Comstock proceeds with extraordinary rapidity. The drifts are advanced steadily at the rate of three, five, and sometimes even eight and ten feet a day, and therefore the ground in which the miners are working is always fresh ground. The low conductivity of minerals to heat forbids the supposition that a rock of 130° Fahrenheit temperature can lose heat sensibly to any depth in the course of twenty-four hours. The shallow holes which were made use of always lay in new ground, and exhibited results which may be accepted with as much confidence as if they were twenty feet or more deep. Very often they were in ground which had been exposed only one or two hours, having been sunk immediately after a blast which threw off four or five feet of the rock. The surface which was thus thrown down had not been exposed more than twenty-four hours. The high temperature and small flow of air in the heading forbid the supposition that any sensible diminution of heat could have taken place at the bottom of the drill-hole under such circumstances. The surface of the rock exposed to the air of the drift was found to be about 123° Fahrenheit, the experiment being made near the "header" or end of the drift. The air itself was found to show considerable uniformity when its temperature was taken under circumstances that were at all similar. In freshly opened ground it varied from 108° to 116° Fahrenheit, and higher temperatures are reported at various points, reaching in fact as high as 123° Fahrenheit in the 1900 level of the Gould & Curry.

The temperature of the air is subject to more fluctuations than that of the rock, for the simple reason that it is artificially supplied to the mine, and varies according to the distance to which it is carried, the quantity, velocity in the pipe, and its initial temperature. All of these elements of the problem vary within wide limits. The initial temperature of the air which supplies a particular drift will, for instance, depend upon whether it is drawn from the surface, the bottom of a shaft, where it is often cooler than above ground, or from some old air-way, where it has had time and opportunity to take up heat.

Nevertheless, even under such variable circumstances as these, the temperature of the air in a new drift does not ordinarily vary much more than eight degrees, and in this variation the length of the drift appears to be the most important factor.

This uniformity of temperature, under such changing conditions is due to the well-known fact that the amount of heat absorbed from the walls of a drift or shaft in a mine depends upon the difference in

the initial temperature of the air and rock. The greater this difference, the greater is the absorption, but as soon as the temperature of the air-current approaches that of the rock, the heat absorption proceeds much more slowly.

In the Comstock mines it is the custom, without exception, to blow the air through galvanized iron pipes, the diameter of which is usually from eight to twenty inches. The size most used is eleven inches in diameter, and the usual amount of air blown is about seven hundred cubic feet per minute, this being the supply for two to six or more men, working in one or two "headers."

In most cases the air is not sent down from the surface, but taken from some point in the incline or at the bottom of the shaft. Its temperature may be assumed at about 85° or 90° F. in summer, though it is sometimes higher than this. Its velocity in the air-pipe is not very far from one thousand feet per minute. From these data it will be seen that we have about fifteen or twenty degrees of heat added to the air, in a period of time varying from one-half minute to two minutes. The iron of the pipe is so thin and its conductivity so great that we practically have a slender current of air moving through a body of hotter air. Even this statement of the case does not exhibit all the opportunities for absorbing heat which are forced upon the air.

The iron receives heat both by immersion in the hot air, and by direct radiation from the still hotter walls. The currents confined in it must be thrown against its sides by eddies, and the air is thus made to absorb heat by contact as well as by the transmission of heat rays through it. It is probably to this cause that the uniformity of the air temperatures obtained at the headers may be attributed. They depend almost entirely on the heat of the ground there and very little upon the temperature of the air at the point of supply.

Drifts that do not exceed two or three hundred feet in length are *usually* not above 110° or 112° F. in temperature and more often they are below this. But when the length increases to 1200 and 1500 feet the temperature may rise to 116° F. without any other change in the circumstances.

So far as my personal experience goes, the latter temperature has not been exceeded in any drift into which a good current of air is blown. By a "good current," I mean one of not less than seven hundred or one thousand cubic feet a minute. Still there is no hesitation in asserting my confidence in the higher temperatures which others have sometimes obtained. The view which I take of the

phenomenon and its cause admits of such exceptional heat at particular points as a rational consequence of the forces at work. But I regard them as exceptional, and believe the average temperature of those drifts which are considered to be distinctively "hot," is usually not above 108° to 112° F., though rising to 116° F., when they are very long.

These limits are, however, not in the least degree true of the water which enters the drifts from the country rock, and also from the lode rocks. That approaches more nearly 150° Fahrenheit. The vast body of water which has filled the Savage and Hale & Norcross mines for more than a year, and from which it is safe to say a million tons of water have been pumped within twelve months, gave me a temperature of 154° Fahrenheit. Even after being pumped to the surface through an iron pipe exposed, in the shaft of the Hale & Norcross, to a descending current of fresh air for more than a thousand feet, and then flowing for one or two hundred feet through an open sluice in a drain-tunnel which discharges into a measuring-box, the water in this box was found to have a temperature of no less than 145° Fahrenheit.

But the water varies in temperature in different parts of the lode, like the rock and the air. In the east crosscut 2000-foot level, of the Crown Point Mine, which is noted for its extreme heat, the water, after flowing for nearly one hundred and fifty feet over the bottom of the drift, was found to have a temperature of 157° Fahrenheit. On the contrary, in other places the water is much less hot, but I believe it is as a rule always hotter than the air, and in many cases it appears to be hotter than the rock is found to be, except in especially hot spots.

HOT AND COLD BELTS.

In giving this short description of this remarkable phenomenon, the fact has frequently been referred to that there are points in the mines which are much hotter than the average. The east crosscut of the Crown Point 2000-foot level, which was temporarily abandoned and boarded up on account of the heat, gave me an *air* temperature of 150° Fahrenheit, the thermometer being thrust through a crack in the boarding. I felt convinced that at the head of this crosscut the heat must be higher than this, and Mr. Balch, foreman of the mine, informed me that it had been proved so.

Another hot spot is in the Imperial Consolidated Mine. The incline there has always been very hot, and near the bottom, above

the sump, but under the shoot, a position which allows of no ventilation except that which is induced by local air-currents, the air must stand at 130° Fahrenheit or higher, though I did not test it. In this mine the Black Dike splits, sending a shoot off to the north-east, and a drift has been run on the two thousand foot level, along the eastern side of this branch dyke.

This proved to be a very hot spot indeed. Rock, air, and water were all so much above the usual limits of temperature even in these hot mines that the work of cutting the drift must have been extremely severe. It might not have been accomplished if the expedient had not been adopted of boarding or "lagging" up the sides of the drift with a double thickness of plank, breaking joints. This confined the water, which poured down the walls, to a tight chamber, and left the main part of the drift for the men to work in with comparative comfort. The lagging remains, and has been carried around into the main drift which is still in active use. Its joints are calked with tow, and, one of these being stripped for me, the steam from the water immediately poured out and proved to be scalding-hot when tested by the finger. I did not, however, succeed in getting a fair reading of the thermometer, because the crack was too small to admit more than the end of the bulb. The thermometer must have been cooled by the evaporation of condensed moisture from its bulb; but, even under these adverse circumstances, the temperature of the stream was taken at 123°.

The Belcher south incline has a hot belt of rock, quite narrow, a short distance above the nineteen hundred station, and similar hot places are found in most of the mines.

I am inclined to the opinion that, as a general rule, these hot areas lie in belts, and are not irregular or promiscuously placed in the mass of East country rock. Where this seems to be disproved by the distance run in the superheated rock, it will probably be found that the drift, or incline, and the hot belt have the same direction.

It is noticeable that the neighborhood of a dike is apt to be hotter than any other portion of the rock. An example of this has already been given in the Imperial Consolidated Mine, where a drift run immediately east of a branch dike is still wet and intensely hot, although opened for about two years. The incline of this mine, which is very hot, is also quite near the Black Dike.

But nearness to the Black Dike is also a characteristic of most inclined shafts on the lode. Some are west of it; some in it for long distances; others east of it. These inclines do not all exhibit

greater heat than the average of the mines, and there must be some special reason for the heat of the Imperial Consolidated incline, which was referred to above.

Hot belts are also found at the contact of the diorite and propylite in the Virginia mines. The diorite is itself in active decomposition, and mines which have carried drifts in or near it are very hot. The Julia has explored a quartz seam which appears to lie entirely in the diorite, and this has proved to be one of the hot belts.

This apparent concentration of the heat in the line of contact of two rocks is not supposed to be due to any thermal or electro-thermal action, but to depend merely upon the fact that in this neighborhood the ground is more broken and the surfaces of the rock increased. These conditions are obviously favorable to the action of atmospheric waters.

Belts of excessively hot ground are not the only noticeable phenomena in these mines. More remarkable still are the belts of unusually *cold* rock. These are fewer in number than the hot belts, but they are strongly marked. They are always wet, and the water that drips through the crevices of the shattered rock that composes them is noticeably cold to the touch, and cools down the air of the drift. Such a wet, cold belt of rock exists on the eight hundred-foot level of the Justice Mine, and there is a very decided change of temperature in passing from one to the other side of it. Lest the low temperature of this spot should be attributed to the water which drains through it from the surface, it is well to add that water drips from the rock in numerous places in these as in most mines, and that usually it is hot, or at least warm.

Other cold belts are found in the mines which are not so cool as that in the Justice, but are perceptibly cooler than the rock at a short distance from them. They complete a well-linked chain of heat phenomena, extending from rocks that are sensibly cold to the touch, and may not have a temperature above 50° or 60° Fahrenheit, through rocks that have the average atmospheric temperature, and those which are as hot as surface rocks ever become in Nevada, to those which have a temperature of 157° Fahrenheit. There is no reason to doubt that the gradation is quite regular, and the transition from the lower to the higher temperature is made through a much larger series of intermediate steps than the accidental thermometer readings taken show.

Finally, in the chain of testimony relating to this phenomenon is to be noted the condition of the rock. Wet places have been spoken

of, but the rock cannot be considered as generally wet. There are water-ways, and many of them appear to reach the surface, but they are of limited breadth, like the belts of hot rock. This water is usually hot, but sometimes cool or tepid.

Very often, usually in fact, the rock is perfectly dry, though very hot. That is the case in all the mines. Wet rock is the exception, and dry rock the rule, through the whole lode. In the drifts cut through this hot, dry rock, the walls of the freshly exposed surfaces are painful to the hand, and the air is often filled with dust. The rock is both hard and tough, but, in spite of its strength, it gives an impression of fine porosity to the touch, due probably to its trachytic character. It often has the odor of clay, but not always. It may be slightly adherent, or the impression of dryness upon the tongue may be due to its heat, or to the fine dust which covers every fragment.

SOURCE OF THE HEAT.

Wherever eruptive or plutonic rocks are found it is quite common to witness evidence, in the breaking out of hot springs, that heat agencies are still active within them, and this phenomenon is so frequently observed that hot springs are often referred to as the last phase of eruptive activity. The heat in the Comstock and other mines similarly situated is quite generally spoken of, for instance, as the feeble remnant of a temperature that once reached the point of rock fusion, but the facts encountered have compelled me to seek another explanation. It is impossible to assemble in an annual report all the data upon which this conclusion is based, but many of them will be given. They have led me to refer the high temperatures encountered in the mines not to the internal heat of the earth, nor to the residual heat of the rocks, which were once melted, but to chemical action now maintained in the erupted rocks.

This action is not a combustion, for the oxidizable minerals in the lode and its accompanying rocks, the metallic sulphides, are little altered. In fact, the total quantity of pyrite and other sulphides is not large for the neighborhood of a mineral lode, but on the contrary, strikingly small, and not sufficient to maintain the heat of the rocks and water, except under circumstances of unusually rapid oxidation. That no metallic oxidation of any moment goes on in these rocks is susceptible of proof. The metallic sulphurets in the rock show little sign of decomposition, and this is true even in layers of the propylite, that are fissured and seamy and drenched

with water, whether hot or cold. In fact, the preservation of the sulphur compounds, in presence of so much heat and moisture, is a noticeable fact, which I have frequently remarked in all the mines.

An examination of such analyses of the mine waters as I am able to find confirms this statement. In the geological view of the Comstock lode which Mr. King has prefixed to the third volume of the report on the Fortieth Parallel Survey, an analysis of water taken from the Savage 600 level is given. This is compared in the following table with other analyses recently published in the Virginia newspapers:

Analyses of Water from the Comstock Vein, taken at Different Levels.

	Savage. 600 ft. Grains per gallon.	Gould & Curry. 1700 ft. Grains per gallon.	Gould & Curry. 1800 ft. Grains per gallon.	Hale & Norcross. Grains per gallon.
Silica	1.77	2.21	4.025	3.500
Sodic chloride	0.13	0.04	1.162	1.327
Calcic sulphate	29.40	14.35	16.683	22.532
Magnesic sulphate	1.77	....	....	....
Sodic carbonate	0 91	....	....	18.518
Potassic carbonate	7.56	6.42	26.199	8.342
Magnesic carbonate	2.98	....	....	....
Alumina and Ferric oxide	0.05	trace.	trace.	trace.

This table shows that the source of the heat cannot be the decomposition of a metallic sulphide like pyrite, for the resulting sulphate would be highly soluble, and the water would be much stronger in sulphuric acid. It is true that sulphuric acid enters more largely into the analysis than any other acid, but even if this is derived entirely from the decomposition of pyrite, the quantity is entirely insufficient to account for the effects. The Hale & Norcross water, for instance, contains only 54,219 grains of solid matter to the gallon of water weighing 58,373 grains, or less than a tenth of one per cent. Of this solid matter only five and a third grains are sulphur, and this quantity corresponds to a little less than eleven grains of pyrite, containing, say 5.3 grains sulphur to 5.2 grains iron.

If these substances were not in combination, the iron and sulphur in oxidizing would give out heat enough to raise 42,462 grains of water one degree Fahrenheit in temperature, or the heat given out

would be sufficient to raise 408 grains of water from 50° F. (the assumed temperature of surface water) to 154° F., the temperature of this great body of water. This calculation omits the loss of heat which would be suffered by the breaking up of the combination of iron and sulphur, as they exist in pyrite. It is only an approximation, but it shows clearly that the oxidation of the iron and sulphur accounts for less than $\frac{1}{143}$d of the heat present in this water.

It is true that the analyses given do not account for the portion of calcic sulphate which has been deposited as an insoluble precipitate in the crevices of the rock. Gypsum is present everywhere, in fact, in and out of the vein, but its quantity is quite limited in the lower levels, and considered as the cumulative result of many centuries of activity, it affords additional proof that the oxidation of the pyrite has been very small in amount.

AMOUNT OF HEAT WITHDRAWN FROM THE ROCKS.

The quantity of water pumped from the mines the past year must have been as much as 350,000 or 400,000 tons a month. If its temperature is assumed to be only 135° Fahrenheit, and the average temperature of the air for the year 50° Fahrenheit, we have in the year, say 350,000 × 12 = 4,200,000 tons of water raised eighty-five degrees in temperature; or as the usual expression is, 4,200,000 × 85 = 357,000,000 ton-heat-units have been absorbed by the water. If the heating power of anthracite coal is estimated at 7500 heat-units to the ton, the heat in this water is as much as would be obtained from the combustion of 47,700 tons of coal. A cord of pine wood weighing 2700 pounds, will probably give about 4300 heat-units in practice, so that 84,000 cords would be necessary to keep up the heat withdrawn from the rocks in the mine waters alone.

If ten tons of air pass through the mines collectively each minute, or 14,400 tons daily, and the air when discharged from the mines has an average temperature of 92° Fahrenheit, the total quantity of air for the year will be 5,256,000 tons, and the average rise in temperature 42 degrees. The specific heat of air being 0.267, we have 5,256,000 × 0.267 × 42 = 58,940,784 ton-heat-units for the amount of heat absorbed by the air. This corresponds to an expenditure of 7859 tons of anthracite coal, or 13,707 cords of wood. The total quantity of heat carried out of the mines yearly by the water and air is therefore 416,000,000 ton-heat-units, to produce

which, in ordinary industrial operations, would require 55,560 tons of anthracite, or 97,700 cords of wood.

The number of men employed under ground in the mines of the upper Comstock is less than three thousand, and the heat from their bodies, together with that produced by the burning of the large numbers of candles, could not account for any considerable proportion of this heat. Indeed, it may be assumed, in the absence of calculations, that all the heat from these and other ordinary sources of heat in mines is no more than sufficient to compensate for the large amount of refrigeration produced by the liberation of the compressed air which is employed in every mine to work numerous underground machines. This heat absorption has not been taken into account in the above calculations.

In another respect, also, these calculations are defective and give results very much too low. Usually the air enters the mine dry and leaves it saturated with moisture, the evaporation of which indicates an amount of heat absorption, which would probably increase the above figures surprisingly.

These calculations, imperfect as they are, show that the source of the heat is one that acts on a magnificent scale, and also that it cannot reside in the small quantity of pyrite which is oxidized. That source is probably the chemical alteration of the feldspathic minerals of the propylite and other rocks. This change consists apparently in the process of transforming feldspar to clay, technically known as kaolinization, from the fact that china clay, or *kaolin*, is produced in this way. Numerous zones in which this process of alteration has gone so far as to produce complete disintegration of the rock are passed in drifts cut into the country rock on both sides of the lode. No analyses of this decomposed material are at hand; those which have been published always being made upon the clays of the lode itself, where the introduction of silica in large quantities has necessarily exerted a dominating influence upon all alterative processes. In the absence of such analyses, it is impossible to say whether the decomposition that has taken place to such a marked extent at a distance from the quartzes, owes any of its force to the special solfataric action to which the filling of the lode may be ascribed, or whether more general agencies have been sufficient to produce the observed effects. Nor can it be declared without such a critical analytic examination, that this great mass of heated rock, extending for miles in length and breadth, and for thousands of feet

in depth, has passed through ages of drainage from the surface without undergoing some general change in its chemical structure.

On the contrary I assume that alterative action has gone on throughout portions of these rocks and is still in progress. The usual explanation for the heat which is found to exist in eruptive rocks in so many districts, namely, that it is the last manifestation of the heat which formerly fused the rocks, is rejected because of the persistence with which the supply of heat is maintained under circumstances that make extraordinary drafts upon it. From data previously given, it will have been noticed that the mines receive about ten tons of air per minute, and raise its temperature from 50° Fahrenheit, which I suppose to be about the yearly average of the atmosphere at Virginia and Gold Hill, to about 92°. This represents a constant abstraction of heat from the rock, amounting to no less than 161,482 ton-heat-units daily, corresponding to the combustion of thirty-seven cords of wood. The real quantity is probably at least ten per cent. greater than this, the difference being represented by the vaporization of moisture in the downward course of the air.

Fortunately for the purposes of this survey, Captain T. G. Taylor, superintendent of the Yellow Jacket Mine, has caused observations to be taken for several months on the temperature of the air current in different parts of that mine. By noting the increase of heat and the amount of air flowing through the drifts, I have been able to obtain an approximate estimate of the amount of heat drawn from the rock surfaces. It is approximate only, because the records of surface temperatures for several months were accidentally destroyed, and I was compelled to replace them from the careful records taken by Mr. B. Gilman, of the Chollar Potosi Mine, which is in Virginia, and has a higher position and a different exposure from the Yellow Jacket. The present calculations are presented merely to show that these observations contain trustworthy evidence that the heat taken up by the air cannot be derived from deep sources by transmission through the rocks, nor from a magazine of heat lying dormant in the strata.

The Yellow Jacket is a downcast mine, and the air current passes down the vertical shaft to the 1119-foot level, thence down the incline to the 1732 level, through a drift to the south winze, and thence down this winze to the 2200 level, the bottom of the mine. On its way from the 1732 it sends a current through the 1935 and 2040 levels, these currents being reunited in the north winze, which

is the upcast. The north winze does not reach to the surface, and no air rises to day in the mine, the entire current flowing into the Imperial and Bullion mines, both north of the Yellow Jacket, and both of them exclusively upcast.

Captain Taylor has placed Fahrenheit thermometers of the common kind, with japanned tin cases, at the surface, foot of the vertical shaft (1119 level), 1732 south and north winzes, 1935 north winze, and 2040 south and north winzes. The observations obtained are extremely suggestive, the plan of the mine being such as to eliminate complications from the single problem of heat absorption by moving currents of air from rock surfaces. From the 1531 level two parallel winzes are sunk on the lode, inclining with it. They are four hundred and thirteen feet apart, and connected on every lower level by the main north and south drift. The south winze is downcast, and the thermometers placed here on the different levels measure the increase of heat in the winze itself, while those which are hung at the north winze measure the increase of heat which each "split" of air gains in moving through 413 feet of drift, that being the distance between the winzes. This fortunate arrangement of the ventilative currents presents the most favorable opportunity I have ever observed for studying the problems involved. The thermometers should be replaced with standard instruments, and the air current measured twice a week for a year, in each drift. The result would be the best series of observations obtainable, probably in any American mine, for the comparative shortness of the paths followed by the air when contrasted with the long drifts of some coal mines, is compensated for by the high temperature of the rocks and the marked increase of heat in the air.

Before giving the table of results obtained during the first half of 1877, it is necessary to say that they are merely tentative. The destruction of the surface readings and the absence of good standard instruments forbid the acceptance of the results as perfectly accurate indications of the heat absorption. Only two of the monthly records which were taken—those for May and June—refer entirely to this mine, the surface readings of these having been preserved. The irregularity due to the introduction of surface temperatures does not, however, affect the underground readings, but merely makes it impossible to correctly gauge the absorption of heat in the vertical shaft. The gain of heat, after the foot of the vertical shaft is passed, is fully given for the whole six months, the records in the lower levels being continuous from that point.

The air-current entering the mine July 2d, 1877, was measured and found to be 18,140 cubic feet. On the 1732 level the "split" or secondary air-current was found to contain 7200 cubic feet, and for the purpose of illustrating the steady flow of heat from the rock, we may reasonably assume that 18,000 cubic feet of air enter the mine every minute, and that this current is divided into three splits of 6000 cubic feet each, which pass from the south winze 413 feet to the north winze, on each of the three levels, 1732, 1935, and 2040. The second of these is out of consideration, from the fact that there is only one thermometer on it, so that no comparison of the initial and final temperatures can be made. From the average temperatures given above we find that the gain on the two other levels was:

1732-foot level, ...	10.56° F.
2040 " "	7.87° F.

Six thousand cubic feet of air weigh nearly 400 pounds avoirdupois, and the amount of heat absorbed in travelling 413 feet through the drift is:

In the 1732 level, . . .	1128 pounds Fahrenheit heat-units.
" 2040 " . . .	840 " " " "

As one pound of anthracite has been assumed to produce 7500 heat units, and one pound of wood 3185 heat units, the heating power of these two drifts is per minute,

For the 1732-foot level, . .	0.150 lbs. coal, or 0.353 lbs. wood.
" 2040 " " . .	0.112 " " 0.264 " "

The reason for the different absorption in the two levels is that the initial temperature of the air in the drifts increases from 78.20° at the 1732 level, to 85.96° on the 2040, by its journey of about 520 feet in the winze connecting the two levels. The absorption of heat by a moving current of air is known to vary with the difference in temperature of the air and the rock surface. The nearer these approach each other in temperature, the less is the heat absorption.

The most conclusive evidence that the incessant drain of heat cannot be maintained by a constant store accumulated in the rock is supplied by the 1732 level. The exact date at which the drift connecting the two winzes on this level was finished, is not in my possession, but as the station in the main incline was completed in July, 1874, it is fair to conclude that the drift was cut through by the end of 1875. This would give one year's exposure of the rock surfaces by January, 1877, the date when the thermometer readings were

begun. The quantity of air flowing through is assumed to be 6000 cubic feet, as in the previous calculations. The gain of heat under these circumstances was as follows, in degrees Fahrenheit and monthly averages:

YELLOW JACKET MINE, 1732-FOOT LEVEL, 1877.

	South Winze.	North Winze.	Gain.
January,	75.62	86.50	10.88
February,	76.85	83 87	7.02
March,	76.10	88.89	12.79
April,	77.48	88 23	10.75
May,	81.42	91.11	9 69
June,	79.39	91.62	12 23
Average for six months,			10.56

As before shown, the heat absorption amounts to 1128 pound-heat-units per minute, which corresponds to the heat from 0.15 pound of coal per minute, or 216 pounds in twenty-four hours.

In this drift then, with an average age of at least a year and a quarter, the rock surface gives out as much heat as would be obtained from coal fires, placed at distances of 100 feet, the whole length of the drift, and each burning 52.3 pounds of coal daily. Only one candle burns constantly in the drift, and the travel does not amount to more than two hundred trips of one man in one direction in twenty-four hours. The effect of this travel is limited to the change of shifts and the transport of rock and timber, both of which are concentrated in short spaces of time. A hoisting-engine in the winze uses about 9000 cubic feet of compressed air daily, at fifty pounds pressure, which is quite sufficient to neutralize all the heat that can be obtained from the transitory presence of men in the drift. To further show how completely this source of heat may be neglected, it is enough to say that only the morning observation, at 6 A.M., is made at or near the time of this travel.

In other respects I have not observed any circumstances which throw serious doubt upon the thermometer readings. The instruments are not standards, it is true, but they are properly hung on timbers, and usually with ten or twelve inches of wood or air between them and the rock surface. Whenever compared with one of the Survey thermometers, hung in the centre of the moving air-current, they have not shown a variation of more than one degree. The daily readings are quite uniform, the fluctuations of more than one

degree not exceeding 23 in a series of about 360 observations. The highest fluctuation noticed is three degrees.

There can be no doubt that the discharge of heat is real, that this heat is constantly removed by the air, and that it comes from the rock. When we regard the two main hypotheses which have been trusted to explain the high temperature so often noticed in volcanic rocks, namely, internal heat of the earth, and residual heat stored up in the rock mass, it is difficult to understand how either of these sources can maintain this enormous discharge of heat for years. The circumference of the drift is about twenty-eight feet, outside of timbers, the dimensions being six feet six inches at bottom, five feet six inches at top, and eight feet high, when the timbers are twelve inches thick. The area of the walls is therefore 413×28, or 11,464 square feet, and the heat radiation amounts to 142 pound-units per square foot in twenty-four hours.

Experiments have been made to ascertain the radiation of heat from a blast furnace smelting iron ores. Its walls were probably less than five feet thick, and within the furnace a furious fire, producing an average temperature, in the upper part of the furnace, of probably 1200° Fahrenheit, was constantly maintained, and under these circumstances the radiation in twenty-four hours was ascertained to be about 145 Fahrenheit pound-units of heat per square foot per hour, or 3450 per twenty-four hours. The heat, as constantly given out by the walls of this drift, is therefore nearly four per cent. of that which the furnace walls radiated. Considering the long exposure of the rock—for one year—and the unremitted removal of heat, it seems incredible that a definite store of heat placed in the volcanic mass, once for all, could furnish the necessary supply. The amount of heat transmitted is inversely as the distance between the source and the point at which it is discharged, so that with a definite store of heat the rock near the drift would cool rapidly, and a layer of cooled rock would be established in a few years which could not give passage to the amount of heat that experiment shows to be constantly given off even in levels that have been open and well ventilated for five years and more. It is on such considerations and facts that I base the suggestion advanced in this report, that the supply of heat is constantly maintained by chemical action of some kind. There is only one kind of chemical action that is known to have gone on to any considerable extent in the Comstock rocks, and that is the alteration of feldspar to kaolin.

The facts here given are held to indicate that this extensive alteration is still in progress.

Even if this conclusion is granted, it still remains to show how the heat produced by this alteration can be poured into the drifts in such quantities. There is good evidence that the chemical action is not confined to the surfaces of the mine openings, for in that case the rock would necessarily flake off and swell in consequence of its superficial alteration. This takes place to a considerable extent, but not enough to account for more than a small portion of the heat, and there are extensive areas where the drifts remain for years without any visible sign of decomposition in the rocks. The principal part of the chemical action must take place in the body of the rock mass, and it is evidently necessary to find some means for the constant conveyance of the heat produced to the artificial openings.

The vehicle for the conveyance of this heat I conceive to be gaseous currents, heated by the chemical action spoken of, penetrating the rocks in every direction, and tending to discharge themselves into any free channel, like a drift, opened in the ground. The source of this gas is primarily the atmosphere. Water is capable of absorbing 0.025 of its own volume of nitrogen, 0.046 of oxygen, and its own volume of carbonic anhydride at the ordinary atmospheric pressure. At a higher pressure the absorption is greater, and as the water in the rocks two thousand feet below the surface is under a considerable hydrostatic head, it probably contains a maximum quantity of gas.

The fixation of this water in the solid form, by combination with the silicate of alumina, necessarily liberates the gas it has dissolved, and this gas must then seek to discharge itself at the only point that is free under natural conditions,—the surface. On its way upward it continually meets fresh supplies of water, and is reabsorbed until the water becomes saturated. As the water would always carry down carbonic anhydride and air derived from meteoric sources, there would be additions at every rainfall to the store of gas in the strata, and the cumulative results of years of this action must be the saturation of the rocks with gas, so that whatever is liberated by the solidification of the water will find no place to rest without pushing out some of the gas already held so abundantly in the interstices of the rocks. From the point where this chemical alteration of the feldspar takes place there must be a discharge of gas at least equal in quantity to the rainfall which reaches that depth, and this stream

of hot gas will take its way to the surface and maintain the heat lost by radiation from the rocks through which it passes.

Though rocks readily yield passage to vapors and gases when dry, the contrary is true when they are wet; then the pores filled with water are closed to the gases. On the Comstock we find areas of wet and dry rock disposed in highly inclined sheets, of which the dry greatly preponderate in thickness, forming, indeed, the main mass of propylite, in which the wet portions stand as isolated layers. Both are heated, and to an equal degree, or at all events the experiments hitherto made have not been careful enough to ascertain any variation, but in the higher heat of the mine waters we may possibly have evidence of some wet bands that maintain a temperature excessively high even for the Comstock.

It is quite possible that the wet layers serve as channels to carry down a reagent, water, which produces heat by chemical combination in the lower depths, and the dry rocks serve to transmit to the surface the gases liberated from the combining water, and bringing the resultant heat with them. Thus the wet rocks are heated by decomposition in place, and the dry rock by the passage of hot gases from that decomposition.

It is, therefore, conceivable that the rapid flow of heat into the drifts is caused by streams of hot gas filtering readily through the porous rock, under the pressure to which it must be subjected, as a consequence of its deep subterranean position. These gaseous currents do not make themselves evident to the senses over the ordinary surfaces of drifts, but they frequently pour out of drill-holes with strength sufficient to flare a candle-flame. "Blowers" of gas are met with, but not with more than usual frequency in mining operations. Accumulations of choke-damp in old drifts are common, though perhaps not more so than in all mines. The mine waters evidently contain gas, which bubbles through them, and sometimes in quantity sufficient to produce a moderate boiling.

None of the analyses quoted above give the amount of carbonic anhydride present in the mine waters, but there are evidences that it must be dissolved in considerable quantities. All water-channels in the mines fill up with a reddish powder, caking but slightly, and sometimes nearly filling conduits that are placed at a considerable distance from the source of the water. In the absence of analysis, this may be assumed to be composed mainly of alumina, lime, iron, and silica, precipitated by the evaporation of the gaseous carbonic anhydride, which is recognized as a powerful agent for the solution

of these substances in water. Similar depositions occur in other mines from this cause, and they indicate decomposition of the neighboring rocks.

The presence of such gas currents affords another explanation of the hot spots observed in the mines. In discussing this phenomenon it was observed that these places lay in belts of rock that are peculiarly susceptible to decomposition, but if the rocks are permeated by currents of hot gas, it is evident that those portions which are most fissured will give the most ready means of exit to the gas, and therefore be kept at the highest temperature. Such may be the true cause of the high heat observed in the Imperial incline, lying along the Black Dike, which is not everywhere exceptionally hot; and also in the northeast drift, on the 2001 level of the same mine, which is run along a branch dike. The Black Dike appears to be, on the whole, less susceptible to decomposition than the rocks which inclose it, but it is frequently accompanied by highly fissured rocks some feet in thickness, a phenomenon common to eruptive dikes. A thin and unimportant seam of quartz is also found at many points in its course, which indicates the presence of a previous crevice, or broken ground. These conditions evidently are favorable to the free passage of currents either of gas or water.

Such currents of gas do not account for the cold belts of rock, which are not dense, but, in every case noticed, fissured and wet, and it is probable that both the causes here advanced—local decomposition of different intensities and gaseous currents—are in action.

It is to be regretted that the casual and frequently intermitted observations upon heat, which were all that the pressure of other duties permitted me, were not so calculated as to take advantage of the accurate results obtained by Sir William Thomson, from Professor Forbes's observations in the trap rock of Calton Hill, Edinburgh. He found that if a plate of this material, one foot thick, had one side kept at a temperature constantly one degree hotter than the other side, the heat which would pass through it, in one year, would be sufficient to raise 144.1 cubic feet of water one degree in temperature; or, 0.3863 cubic foot in twenty-four hours. But the amount of heat transmitted varies as the thickness of the plate, and it would be necessary to ascertain accurately the heat at different depths, in the same locality, before the true rate of transmission in the Comstock drifts could be settled. Only one observation of the surface temperature in a drift was made. With a temperature of 136° F., in a drill-hole bored in the freshly exposed rock of the heading, and an

air temperature of $110\frac{1}{2}$° F., in the drift, a thermometer laid on a projecting point of the wall, fifteen or twenty feet from the heading, showed 123° F. This surface must have been about four days old. The reading gives a certain amount of precision to the expression of a fact well known to the miners,—that the rock cools rapidly in the first few days of exposure.

But while the observations made in these mines are neither exact enough nor complete enough to permit general deductions on the basis of Sir William Thomson's formulæ, it is possible to make sufficient use of them to demonstrate the fact that transmission alone cannot produce the results observed. Between the 2200 and the 1732 levels of the Yellow Jacket Mine there lies a stratum of rock 468 feet thick. Its lower surface is constantly maintained at a temperature which is, taking the mean of two readings, 138° F., and from the walls of a drift opened at its upper surface there is a constant absorption of heat by an air-current, amounting to 142 pound-heat-units per square foot and twenty-four hours, this being the mean of a six months' period of observation.

Since the transmission of heat through a one-foot stratum of rock, having a difference of one degree between the temperature of its upper and lower surfaces, is sufficient in twenty-four hours to heat 0.3863 cubic foot of water, one degree Fahrenheit, for each square foot of surface; we have, $0.3863 \times 62.447 = 24.123$ pound-heat-units, transmitted through eruptive rock in twenty-four hours, per square foot of surface, under these conditions. For a stratum 468 feet thick, the transmission, being inversely as the thickness, would amount to 0.0515 pound-heat-units, per square foot for each degree of difference in temperature between the upper and lower surfaces.

To maintain the radiation of 142 pound-heat-units, per square foot of surface on the upper level, at this rate of transmission, would require a temperature of $142 \div 0.0515 = 2757$ degrees Fahrenheit on the 2200 level.

Mr. Charles T. Hoffmann found the following temperatures in the 1531 level of the Yellow Jacket Mine in 1873:

Dry hole in upper part of the header, ...	123° F.
Wet hole in lower part of the header, ...	120° F.
Air in the drift, ...	102° F.

Taking the difference between the heat of the dry hole on this level and that found on the 2200 level, or $138° - 123° = 15°$, we have an increase of fifteen degrees in 670 feet of depth. This gives

an average rate of increase amounting to one degree for 45.67 feet, and this is so near that determined by me for the whole ground (one degree to 45.45 feet), that for the purpose of this calculation we may safely take one degree to 45.5 feet of descent to be the rate of increase for this mine. By this there should be an increase of 4.4 degrees between the 1531 and the 1732 levels, and the temperature of the latter would be that of the 1531 *plus* the increase; or $123^\circ + 4.4^\circ = 127.4^\circ$. Therefore, the difference between the temperature of the 1732 and the 2200 levels is nominally $138^\circ - 127.4^\circ = 10.6^\circ$. Our rock stratum of 468 feet thickness accordingly has its lower surface constantly maintained at a temperature 10.6° higher than its upper surface. Its rate of transmission being 0.0515 pound-heat-units, per square foot and twenty-four hours, the radiation on the 1732 level should be $0.0515 \times 10.6^\circ = .05459$ heat-units. This is only 3.84 per cent. of the radiation which is found to be actually taking place.

The case may also be stated in another way. If the transmission through a one-foot stratum is 24.123 pound-units per square foot, in twenty-four hours, for each degree of difference in temperature, and the radiation on the 1732 level is 142 pound-units for the same surface and time, it is evident that a rock stratum having its lower surface kept 10.6 degrees hotter than its upper surface, could at this rate not exceed a thickness of 1.8 feet. For a stratum of this thickness would transmit 13.4 pound-heat-units per square foot in twenty-four hours, and $13.4 \times 10.6^\circ = 142$ units for the radiation. Instead of the height of 468 feet between the 2200 and 1732 levels, the law of transmission would not admit of a stratum more than twenty inches thick between the surface where a radiation of 142 foot-pound heat-units is in progress and the surface where an excess of 10.6° F. is maintained.

It is true that the accuracy of these results is vitiated by the fact, that the walls of the drift represent a little more than four times the real upper surface of the stratum, but even if allowance is made for this only sixteen per cent. of the total radiation in the drift can be accounted for by mere transmission. Rude as the calculations are, they establish the assertion that some other cause for the passage of heat through these rocks must be sought.

It would be interesting to learn the rate at which heat passes through these rocks under the combined operation of gaseous currents and ordinary transmission. Ample opportunities exist for trustworthy experiments upon the subject, for there are scores of

diamond-drill bore-holes, each some hundreds of feet in length, on different levels in all parts of the lode. But no data have been collected even for such rough calculations as I have ventured upon in this paper.

On this account it is equally impossible to apply, at present, Joule's discovery that gases do not lose heat, or not so much heat, in expanding when they pass through a porous plug as when they are delivered freely. The rock evidently acts as such a plug to any gases contained in them. Nevertheless, the fact is, it does cool rapidly; its rate of transmission, excessive as it is by Sir William Thomson's formula, not being sufficient to maintain its temperature with an air-current passing. To what depth this refrigeration extends is not known.

It is easy to see that the presence of gaseous currents in the strata of the earth is not confined to the region of the Comstock lode, but must take place wherever meteoric waters are carried into low-lying strata, and there enter into chemical combination with mineral substances. The regenerative action by which each dose of rain-water intercepts the escaping currents of gas, as they rise through the rocks, and adds to the store it has brought from the surface, until the point of saturation is reached, must also go on wherever hydration takes place in underlying rocks. The extreme discrepancies in the records of subterranean rock temperatures, so far as they have been observed, have never been explained, but it seems probable that wherever kaolinization is taking place, the temperature, both of the strata affected and of those overlying them, will be raised.

It has been pointed out above that the susceptibility of the Comstock rocks to feldspathic decomposition cannot be considered a peculiarity, since volcanic areas in other places, exhibit similar heat phenomena. All the rocks, propylite, diorite, and andesite, show this tendency to break up under the action of water. On the surface there are areas where the alteration has been deep, and others of just as ancient exposure, where hardly anything more than discoloration has taken place, the material remaining firm. A similar state of things is found below the surface, and at all depths reached by the works. Soft seams, hard rocks, which remain hard through years of exposure, and hard rocks which flake down and soften under the action of the air, are all met with, and apparently are not distinguished by mineralogical differences.

Upon examining these varying layers, the conclusion seems irresistible that the very soft rocks have been the channels of the me-

teoric waters, and perhaps also of rising mineral waters, while the solid and unaltered ones have not been the seat of water movements to any great extent. This observation makes the heat of the hard rocks all the more noticeable. They are hot, and sometimes rock which is perfectly solid, both before and after exposure to the air, is as hot as any of the layers met with, though, as a rule, the more decomposed seams are either colder or hotter than the unchanged layers.

In the decomposed layers which I have supposed to be the channels of the rising solfataric waters, the clay exists in a state of incomplete hydration. It absorbs water with great avidity, and under such circumstances develops unusual heat. When drifts are opened in such material, the clay absorbs moisture from the air currents, swells, and throws off flakes of rock which would come down indefinitely if they were not kept back by timbers. Sometimes, the breaking of a pump allows water to rise upon such a stratum, and then the swelling is so great and forcible that timbers twelve and fourteen inches thick are broken and split into small pieces.

These decomposed layers may be looked upon as the furnaces of the district. They are found on both sides of the vein and in all the rocks of the region. Sometimes widely separated, they also frequently lie within one or two hundred feet of each other. Considering the low specific heat and conductivity of rocks, it is not surprising that a temperature of 130° Fahrenheit should have been reached and maintained at depths of fifteen hundred feet and more, where radiation is necessarily slow.

Whatever the exact mode of decomposition is, it is certain that the result is the same as if the Comstock mines were excavated in a mass of burning material. At present the source of the decomposition is probably atmospheric action. For a certain distance from the surface, which may be approximately put at 1000 feet, the process was complete or nearly so, before the advent of man. The fire had burned out, and the rock had cooled down.

When this zone of burnt-out rock had been passed in the mines a lower zone was entered, and here the fire was found to be still in progress. It is not unfrequently said that the heat of the mines has not increased as they have deepened, and in proof of the assertion it is reported that the first bodies of hot water struck were nearly as hot as those that have been tapped at lower levels.

I am inclined to doubt the strict accuracy of this statement, though it is quite possible that isolated bodies of water, tapped on

the upper levels, may have had high temperatures, and been derived from portions of the country rock that were in a more vigorous state of chemical action on that level than the general mass. But the average heat of the water has increased in going lower. Mr. King, as before quoted, gives 70° to 75° Fahrenheit as the average from the surface down to the 700 foot level, and 108° Fahrenheit as the maximum in the lowest workings of the Empire, Crown Point, and Hale & Norcross, which at that time (1869) were about 1000 feet below the surface. This conclusion was formed after a careful comparison of numerous observations. It is probable that after passing through a certain thickness of rock, in which alteration had nearly ceased, the average temperature has increased with some uniformity to its present stand-point.

FUTURE INCREASE OF THE HEAT.

This brings me to the important question: Will the heat continue to increase? If it does increase, will it rise to the point of boiling water?

The elements of the problem are too vague for a definite opinion on this subject. We do not know how rapid the alteration of the feldspar is, how much heat it produces, nor how much surface-water reaches the hot ground yearly.

The temperature of the water in some of the springs at Steamboat, twelve miles distant from the lode, is sufficient local proof that the rocks of the region are capable of producing heat enough to raise a considerable quantity of water to the boiling-point, and at a certain depth this may be the temperature of the rock at the mines. It seems probable, however, that this depth is one that will not be reached by mining in this century, if ever.

In discussing this question it is necessary to keep clearly in mind the fact that there are two distinct classes of hot rocks encountered. One is the ordinary rock, which forms much the greater part of the ground worked in. The other contains a number of individual belts, or bands, of hot or cold rock, which are found inclosed in the former. The nature of these peculiar bands will be considered hereafter. It is upon the condition of the first class, or the rocks which compose most of the mining ground, that a discussion of the general temperature must be based. If we assume the rate of increase in these rocks to be uniform from 108° Fahrenheit at the 1000-foot level to 130° at the 2000 level, it represents 2.2 degrees Fahren-

heit for 100 feet of depth, or 1 degree to 45.45 feet. The lower of these temperatures is that assigned by Mr. King to the "lowest workings of the Empire, Crown Point, and Hale & Norcross," and its depth at the time he wrote was between 1000 and 1100 feet. Mr. King gives this as the maximum temperature, and from the table of observations published by him it is probable that the average may have been in the neighborhood of 100° Fahrenheit. On the other hand the temperatures on the 2000-foot level and below that were sometimes 136° and 139° Fahrenheit. Assuming it at 140°, in order to obtain a maximum, we have a difference of 140°–100°, or an increase of 40° in 1200 feet of depth; an average of 4° to 100 feet, or 1° to 25 feet.

The great difference between this minimum and maximum calculation shows how untrustworthy such estimations are when based upon a limited number of observations. The uncertainty is greatly increased by the differences in temperature which exist within small distances. When such calculations are presented in this report they are to be accepted rather as illustrations than as positive estimations. It is also worth noticing that these rates of increasing temperature, taken in the hottest mines in the world, and where the heat is almost entirely due to natural causes, and not to the artificial conditions of bad ventilation, crowded mines and combustible material, do not much exceed those which have been reported in numerous other localities where no remarkable circumstances were noted. The higher rate, 25 feet to 1°, is perhaps excessive, but many determinations in ordinary rocks have given as much as 45½ feet.

The greatest depth to which engineers of the present day are looking forward as the maximum to which they may be called upon to sink is from 4000 to 6000 feet. The former depth will no doubt be reached by the present generation, but I believe preparations for sinking to the latter distance have not yet been made in any country.

If the heat of the general mass of rock increases at the rate of one degree for 45.5 feet, and the temperature at 2000 feet is 130° Fahrenheit, the increase for 2000 feet more would be 44° and the temperature, at the 4000 foot level, would then be 174° Fahrenheit. At the 6000 foot level it would be 218°.

By the maximum rate of increase calculated above, or one degree to 25 feet, the temperature at the depth of 4000 feet would be 210°, and at 6000 feet depth 290°.

It is, however, a matter for grave doubt whether under the existing circumstances of the Comstock region the rock will be found at

any depth to have the temperature of 212° Fahrenheit. The access of atmospheric air and water must decrease proportionately to the depth, after a certain point is reached, and at that point the decomposition of the rock and the heat produced will be at a maximum.

The opinion that the heat will increase from its present standpoint, is a necessary deduction from the explanation which I give of the whole series of phenomena. I cannot agree with the conclusion of Mr. King, that the heat of the rocks is brought in by water that rises from great depths and in a heated condition. I consider that the reverse action is the one which is really going on. The rock heats the water.

The temperature of the rock was determined by a number of trials to be 130° Fahrenheit, as stated above, while water in large quantities comes into the workings, exhibiting a temperature of 154° Fahrenheit. According to the hypothesis here advanced there must be rock of this temperature somewhere in or near the lode. If the water is assumed to rise from great depths and the rate of increase is uniformly one degree for 45½ feet, the Savage and Hale & Norcross water of 154° temperature must come from the depth of about 3100 feet, if the temperatures taken in the Crown Point 2000 level are assumed as a basis for calculation. Unfortunately it was not possible to obtain data from the flooded mines, as their lower levels were covered by several hundred feet of water during the whole of the field season. The depth of 3100 feet is 900 feet below the level on which the water was struck.

Since the theory that the rocks are heated by water drawn from great depths has been rejected in this report, it is necessary to explain how it is that the water coming into the mines is almost uniformly hotter than the rocks, except when it is palpably surface water, filtering down through a shattered seam.

It seems to be pretty well settled that the waters of the lode occupy strata of rock which are generally parallel with it, these strata being separated by clays, dikes, or solid strata which are impervious to water, and accordingly retain the liquid between them. The numerous long drifts run to the eastward, cut these parallel strata in great numbers, and tap the water they contain, sometimes in formidable quantities. But in spite of the number of these drifts, it is probable that some bodies of water have never been drained, but remain in the country rock at levels much above the lowest workings of the mines.

I consider these water-bearing bands of rock lying parallel with the lode, to be identical with some of the hot bands previously

described, and a little consideration will show that the seamy and shattered condition which fits a rock for storing up water in large quantities also increases its susceptibility to decomposition by increasing the surfaces acted on. The vast quantities of water which have again and again suddenly flooded one mine after another are evidence that the reservoirs which contain them are also made up of shattered seams, for the undecomposed rock of this region is finely porous, and not coarsely porous. It could hardly contain as much water as an ordinary sandstone, and certainly it could not give up its fluid contents with any great rapidity when in its natural condition. These facts have been recognized for a long time in the Comstock, where there is now a settled conviction among intelligent observers that the great inflows of water come from shattered and decomposed seams, parallel with the lode, and sometimes of great thickness. The old idea that the country rock contains cavernous openings holding large bodies of water has been abandoned, in consequence of the proof afforded by more than a hundred miles of exploratory workings, that the inclosing rocks are singularly free from vugs and open crevices. Very few have been found in proportion to the ground penetrated.

These facts force upon us the conclusion that the high temperature of the floods of water is not necessarily due to the depth of their origin. It is more probable that the hot waters come from seams which, on account of their shattered condition, are both more susceptible to chemical action and also more capable of storing up the fluid. They are the "hot belts" of the country rock. Not only is the deep origin of the hot waters not proved by the facts, but the contrary hypothesis is strongly borne out. When they are encountered, it is quite a common experience to strike them *below* the highest point of their source, and the mine is often flooded by such an occurrence to the depth of several hundred feet. The case of the Savage and Hale & Norcross mines is quite in point. The vast accumulation of water which has disabled them for nearly two years, was first struck in the 2200 level of the Savage and filled the two mines to the 1700 level, a height of 500 feet, besides filling the 2400 level of the Savage. The pressure which lifted this water through 500 feet of height cannot be attributed to the tension of any gas, for it has been shown in a former part of this report that the emanations of gas in the mines have never been very forcible.

The head of 500 feet under which this water entered the mine, and which was steadily maintained for several months, during which new pumps were put in, may be referred with the most probability

to a simple hydrostatic pressure. Its origin was not deeper than the 2200 level, but, on the contrary, at least 500 feet above it, and this fact, taken with its temperature of 154° Fahrenheit, forms the most significant evidence in regard to the origin of all these bodies of heated water, for they present similar phenomena.

The uniform disposition of the rocks in the Comstock region, entirely unbroken as they are by faults, gives strong probability to the supposition that these collections of water extend to great depths. I suppose that there is a certain amount of convection in this water; currents from the upper portions of the strata setting downward and pressing up currents of hot water from lower depths. This action maintains the water in the upper parts of the strata at a higher temperature than it would otherwise have. The hot water drawn from the lower depths presupposes the existence there of rock of equal temperature. In fact, as there is a constant loss of heat near the surface, we must look upon the highest temperature observed in a large body of water, 154° Fahrenheit, as a mean obtained by mixing colder water from the upper rocks with hotter water from those below. The limited rainfall of Nevada would, however, make the yearly accession of water very small when compared with the quantities which we may suppose the rocks to hold as a permanent store, and this yearly addition would be heated, probably above 130° Fahrenheit, by the heat of the upper rocks, and the fresh chemical action set up by its presence.

From all these considerations it is judged that until water temperatures above 154° Fahrenheit are observed, we may content ourselves with the belief that nothing in the present condition of things indicates the certainty that the heat will ever rise to the boiling-point of water, 212° Fahrenheit. As above stated, it is rational to suppose that the access of atmospheric air and water must diminish in proportion to the depth after a certain point is reached. At that point the temperature will be at a maximum. Below it there will be a state of equilibrium, probably for a very considerable depth. Below that the heat may diminish even to a point below that of the highest of the three zones. There must be some point where the absence of drainage allows the water to lie like a blanket over the rocks, protecting them from the action of air or gases from the surface. The known depth required for the production of a temperature amounting to 130° Fahrenheit, is so great, that we may fairly doubt whether air or water penetrate to lower depths in quantity sufficient to maintain mineral decomposition with the activity necessary to obtain the boiling temperature.

The boiling heat of the water at Steamboat Springs is evidence that the chemical action is going on there near the surface. The common impression that these springs are the last indications of the volcanic forces which poured out the torrents of melted rock that now cover this whole region, may be accepted in a modified form. The rocks of that neighborhood may be the last of a series that certainly occupied an enormous time for their ejection, and in that sense they are the last to undergo decomposition. Chemical action is nearer the surface in them, because it is newer. Its maximum line is higher than in the older rocks of the series, which are those that now inclose the lode. It is also quite possible that decomposition near the surface may be intensified by the action of organic acids drawn from the soil.

To recapitulate briefly the facts here given, this explanation of the heat phenomena connected with these remarkable mines therefore supposes the existence of a cold, and what may be called a burnt out, layer of rocks, extending for a thousand feet below the surface, a zone of hot rock still in active decomposition, which has been found to exist for a depth of about fifteen hundred feet more, and no doubt extends thousands of feet further, and, finally, a mass of cold rock at a great depth, which has not yet begun to decompose. This hypothesis will be found to satisfy all of the observed facts.

The peculiar bands of hot and cold rocks which have been described, are simply layers of rock in which decomposition has been delayed or hastened. When the texture of a rock is such that it resists decomposition longer than other layers in its neighborhood, it will be at its maximum temperature long after its fellows have passed theirs and cooled down, and this I conceive to be the situation of the hot bands. They are individual layers of rock undergoing delayed decomposition.

On the other hand, when a rock is peculiarly susceptible to the action of the air and water, its alteration will proceed more actively than that of the surrounding rock. It will, therefore, pass its maximum temperature sooner, and be cooled down by the time that its neighbors begin to be at their hottest. This is the state of the cold bands. These bands, in fact, offer us at several places in the mines, examples in miniature of the action that is going on upon a grand scale throughout the whole system of rocks.

All the known facts strengthen the supposition which is advanced in this report, that the heat in the mines is subject to a steady and moderate increase as their depth is increased, this comparatively

regular progression being broken by the passage through belts of rock heated above the average of the "country."

In regard to the single mode of heat production suggested here, it is, of course, possible that other forms of mineral alteration than the transformation of feldspar to clay may have taken place, but they have not been observed. The minerals of the country rock and vein appear to be the same in depth as near the surface in the cold zone. The rocks are remarkably free from zeolites and other evidences of mineral interchange through the medium of water. Whatever has been done of this kind is almost confined to the deposition of quartz in the upper mines, and of quartz and calcite in the lower mines of Virginia and Gold Hill, and to the formation of clay. Calcite is found in the Black Dike and in rocks of apparently similar composition east and west of the vein, and gypsum also occurs on the surfaces of fissured seams; but the quantity of both is so small, excepting in the Devil's Gate part of the district, that they might well be referred to the ordinary action of atmospheric waters in any series of strata.

RELATION OF TEMPERATURE TO DEPTH.

The temperature of the rock has no relation to fixed levels. There do not appear to be horizontal zones in the earth, with a temperature peculiar to each one. But the relation of temperature to depth from the surface is apparent through all the variations due to differences in the rocks and other causes. The depths given are reckoned from the top of the present shaft or some old one in each mine, and there is no common datum assumed. The altitudes of the shafts vary to the extent of several hundred feet, but there has been a certain correspondence (though not a close one) in the depth at which hot water was tapped in the different mines. That is to say, the depth from the varying outline of the surface is the only one that can be called constant.

This is illustrated by the case of the Justice Mine. The mouth of its shaft is nearly a thousand feet below the Gould & Curry croppings, and its lowest workings were about one thousand feet below the surface. These workings are, therefore, within a few feet of the same absolute level as the Gould & Curry 1900-foot station and drift, where the remarkably hot ground above referred to is reported to be. In the latter mine, an air temperature of 123° Fahrenheit is reported in the drift, but in the Justice the rock is still quite cool, and though the temperature of the mine is somewhat higher than on

the surface, this increase is mainly due to the burning of candles, heat of the workmen, and other causes that raise the temperature of most mines.

What the future of the Justice in regard to temperature may be cannot be positively foretold, because the filling of its ore bodies is entirely different in character from that of the upper crevice. It is carbonate of lime, while the whole series of mines from the Utah to the Caledonia have quartz for their gangue. One is basic, the other acid. Still, as the heat is derived from the decomposition of the country rock, and depends upon the lode only so far as to be increased by the numerous surfaces exposed to atmospheric influences near the main channel, it is to be expected that the Justice and other mines in that neighborhood will, in sinking a few hundred feet more, take their places among the hot mines of America. The basic character of the gangue probably cannot of itself affect the result, and is to be regarded only as an indication of conditions in the formation of the lode which may have lessened the liability of the country rock to decomposition.

The depths referred to in this report are all vertical, and for that reason they do not represent the path which the atmospheric agencies have actually taken. That is to be measured along the dip of the rocks, which varies from 40 to 60 degrees. No other course is open to surface waters, which have been clearly proved to be limited in their lateral spread by the numerous clay seams which abound, both in the country rock and in the lode, and which generally dip with the other rocks. My own observations have convinced me that these waters may also be confined by sheets of massive and comparatively impermeable rock, of which there are countless exposures in the east crosscuts of the mines.

We must then consider that the path of these decomposing agencies has been followed, not only for a depth of 2400 feet in the Savage mine, but for a depth (on the dip of the rocks) that is approximately one-half greater than this, or nearly four thousand feet. For more than one-third of this distance the action has passed by; the chemical activity of the rock has ceased, though it is not exhausted. At the bottom of the lower portion, or four thousand feet from the surface, the decomposing action may perhaps be considered to be approaching its maximum.

These distances do not exceed, nor in fact equal, those which have been indicated by theory as the possible depths of atmospheric penetration. Observations upon volcanic action and deep artesian

wells have familiarized us with the idea that chemical action, due to the presence of atmospheric air and water, is going on at the depth of thousands of feet; but no instance of this action at depths greater than those exhibited by the Comstock mines has been pointed out.

The Comstock mines, however, offer a greater promise of discovery in this matter of rock temperatures than any other that I am acquainted with. The extraordinary rapidity with which their operations are prosecuted, the extent of the works, and the fact that they open to inspection a great eruptive mineral lode thoroughly for two miles in length, and partially for many thousand feet more, give them unusual value as a field for investigation. They not only follow an eruptive dike throughout its course, but they also explore a parallel system of eruptive rocks by crosscuts, which are often from 300 to 500 feet long, and sometimes stretch out to 1000 feet and more.

They are also certain to be opened to much greater depths than now, and with a rapidity that will no doubt make them foremost in deep mining within a few years. These conditions, combined with the peculiar susceptibility of the country rock to decomposition, give good reason for expecting that they will before long be the scene of thorough and perhaps conclusive studies in this interesting subject of earth temperatures. This was but a secondary part of my own work, which was chiefly confined to a geological study of the lode, and the many imperfections in this sketch are partly due to this fact.

NOTE ON ZIRCONS IN UNAKA MAGNETITE.

BY PROF. WILLIAM P. BLAKE.

THE magnetic iron ores of the Rees & Wilder tract, Unaka Mountains, East Tennessee, and North Carolina, so far as examined by me, are peculiar in containing considerable quantities of the mineral *zircon* in dark red-brown crystals, ranging in size from one-tenth of an inch, or less, to five-tenths of an inch in length. As zircons are decomposed by fusion with alkalies, the constituents may exert some favorable influence upon the iron product, which is generally acknowledged to be of superior quality.

NEW HAVEN, April 20th, 1878.

MEMORANDUM RELATING TO THE CONSTRUCTION ACCOUNT OF THE RAIL MILL OF THE EDGAR THOMSON STEEL COMPANY, PITTSBURGH, PA., 1874–75.

BY P. BARNES, PLAINFIFLD, N. J.

THE sums given in the accompanying table are those actually paid for material and labor up to about August, 1875; but, as some parts of the machinery were not wholly completed at the starting of the works in September, 1875, the statement cannot be taken as an absolutely correct note of the total cost.

A complete description, with engravings of the building and machinery, was given in *Engineering*, April 26th, 1878, in a paper by Messrs. A. L. Holley and Lenox Smith, to which convenient reference may be made. A detailed list of the construction accounts has been given in a paper, by the present writer, in vol. vi, Transactions of the Institute.

A summary is given in the table of the items in each class which enter into the total cost, and also a note of the proportion which each of these items forms of the whole. A concise view may thus be obtained of the facts embodied in the statement as a whole, even though it cannot lay claim to completeness in showing the entire or final cost.

An urgent suggestion has reached the writer from a distant country that such statements of cost to be really most serviceable should give the details in days of labor, in weight of iron, and in number of brick, and to this an assent is most cordially yielded. It is true, however, that the mere labor of copying and compiling these details to such a degree of minuteness, would be very great, and, in any event, the probable difference between the plans of an existing establishment and one projected, would be so wide as to render even so complete a statement of cost of little more real service than the approximate notes given herewith.

Items.	Class.	Building.	Engine and train foundations.	Rolling mill engines.	Blooming train.	Rail train.	Small engines.	Hot bed.	Hammer	Cranes.	Hand tools.	Scales.	Floor plates.	Totals.	Proportions.
		15	16	17	18	19	20	21	22	23	24	25	26		
1	Cement	$374	1,139	......	80	......	31	......	769		......		......	$2,393	.012
2	Concrete	39	872	......	85	......	......	......	1,424		......		......	2,420	012
3	Rubble	1,718	......	......	1,464	381	309	771	......		......		......	4,643	.024
4	Timber and piles	......	......	......	......	......	......	......	585		......		......	585	.003
5	Red brick	648	3,796	......	......	......	316	......	327		......		......	5,887	.025
6	Bricklaying	......	1,205	......	......	......	......	......	174		......		......	1,379	.007
7	Iron frame	38,015	......	......	......	......	......	......	......		......		......	38,015	.194
8	Roof covering	6,980	......	......	......	......	......	......	......		......		......	6,980	.035
9	Engines	......	......	33,682	......	......	......	......	......		......		......	33,682	.172
10	Roll trains	......	......	......	32,738	24,188	......	......	......		......		......	56,926	.295
11	Hydraulic fixtures	......	......	......	451	......	850	......	......		......		......	1,301	.007
12	Cranes	......	......	......	......	......	......	......	......	Incomplete.	......	Incomplete.	......	......	......
13	Hammer and anvil	......	......	......	......	......	......	......	6,633		......		......	6,633	.034
14	Shafting and belts	......	......	......	......	......	1,290	......	......		......		......	1,290	.007
15	Saws	......	......	......	......	......	4,060	......	......		......		......	4,060	.020
16	Presses and drills	......	......	......	......	......	5,163	......	......		......		......	5,163	.026
17	Bar iron and steel	201	......	18	200	......	67	490	131		1,322		......	2,429	.012
18	Castings	328	......	......	987	......	383	1,913	89		179		4,874	8,753	.044
19	Lumber	1,924	......	15	10	17	307	150	168		46		10	2,647	.014
20	Hardware, etc	255	12	256	155	179	216	59	27		144		......	1,303	.007
21	Teaming	335	389	38	72	......	13	......	12		......		......	859	.004
22	Common labor	355	......	128	199	510	343	202	240		130		128	2,235	c11
23	Skilled labor	954	109	618	1,004	1,518	1,116	338	397		743		42	6,839	.035
	Totals	$52,126	7,522	34,755	37,445	26,793	14,464	3,923	10,976		2,564		5,054	$195,622	1.000
	Proportions	.266	.038	.177	.203	.137	.076	.019	.048		.012		.024		

NOTES ON THE RESULT OF AN EXPERIMENT WITH THE WHEELER PROCESS OF COMBINING IRON AND STEEL IN THE HEAD OF A RAIL.

BY W. E. C. COXE, READING, PA.

MANY of you who are interested in the manufacture of iron and steel, have no doubt heard of the "Wheeler process for combining iron and steel."

Mr. Wheeler has formed a company, styled the "Combination Trust Company," whose business is to grant licenses for the use of this patent process in producing what he calls "iron-clad steel." It has been proposed to apply it in the manufacture of car or wagon axles, also to shafting, having steel cores or centres covered with iron; to tubes of steel with an inner and outer skin of iron; to structural purposes, such as girders and beams; bridge and tie-bolts, where the strength of the iron and the rigidity of the steel would be desirable.

In answer to a request from Mr. Wheeler, permission was accorded him to make some experiments at the rolling mill of the Philadelphia and Reading Coal and Iron Company, at Reading, Pennsylvania, with a view to manufacturing some steel-headed rails.

In making this kind of a rail two difficulties are encountered. If the iron is brought to the requisite heat to weld properly, the steel is likely to be burned; if the steel is cared for, to prevent burning, the several layers of iron are not brought up to the proper welding heat.

Mr. Wheeler claimed that the steel slab covered with iron which he furnished for the head of the rail could not be injured by any heat to which it might be subjected in the ordinary course of heating the rail-pile. He claimed, also, that by this method the porosity, which he thought was characteristic of all solid rails of Bessemer steel, would be overcome. Four steel slabs were furnished for this experiment, being Bessemer steel of .61 carbon, made at the Edgar Thomson Steel Works, and covered with iron in one of the Pittsburgh mills. The slabs were in section seven inches wide by three inches thick, and the iron covering was one-eighth to three-sixteenths inch thick. Each slab was placed on a pile of iron made up in the ordinary way for a rail fifty-six pounds per yard and thirty feet long.

The heating was under the personal direction of Mr. Wheeler, and

his advice was to treat the rail precisely the same as if entirely of iron; that we could neither burn nor destroy the steel. The result was, I regret to say, that four rails were produced with very defective heads, whilst the necks and flanges were sound. After being allowed to cool, the rails were broken to show the fracture, and under the pressure of the straightening press the heads were as brittle as glass. The steel, as will be seen from the specimen shown, is evidently burned, and presents a spongy appearance.

I have not presented this sample, and made these remarks, with any intention of reflecting on the merits or demerits of this practice of making steel-headed rails, but rather with the view of eliciting a discussion from the members present as to the practicability and desirability of thus combining iron and steel.

DISCUSSION.

MR. HOLLEY remarked that it was probable the failure of the experiment at Reading was due to the steel not being entirely covered by the iron.

MR. COXE: The steel slabs were sent to us by Mr. Wheeler already covered with the iron, and, as I understood, they were prepared under Mr. Wheeler's own supervision. I supposed he had taken the precaution to have the work properly done, rather than run the risk of failure by neglecting so vital a point. If it is necessary to have an absolutely air-tight casing for the steel, while it might prove experimentally to be all right, I doubt if it could be made a commercial success.

DR. RAYMOND remarked that the New Jersey Steel and Iron Company had had very considerable experience in the manufacture of steel-headed rails, and thought Mr. Slade might be able to give some data as to the relative wear of steel-headed and solid steel rails.

MR. SLADE said that the steel-headed rails made by the Trenton works were exclusively puddled steel-headed, it having been found that with this steel a reliable weld to the iron could be obtained, the point in which all attempts to make steel-headed rails from Bessemer steel had hitherto failed. As to the life of solid steel rails, he could give from memory no accurate data, but might mention that steel rails, probably of about sixty pounds per yard, laid on the main line of the Pennsylvania Railroad, in Trenton, in 1868, were taken up in February, 1877, after a service of say 8½ years. As to the puddled

steel-headed rails referred to, the results obtained on the Erie, and Philadelphia, Wilmington, and Baltimore roads had shown that their capacity was equal to a traffic of 350 to 400 million tons at one mile per hour, or say 35 to 40 million tons, at ten miles per hour, an amount from twice to two and a half times that borne by good iron rails.

MR. COXE: We have made numerous experiments with puddled steel-headed rails, but they have never given satisfactory results; the difficulty was not in the welding to the iron, for the union was perfect, and they heated and rolled easily, but from want of density and hardness of the steel in the finished rail; they soon gave out by spreading of the steel under our heavy traffic, showing that the steel was too soft. It did not break, or sliver, or peel, but mashed out like lead.

MR. HOLLEY said that the welding heat of iron was sufficient to nearly melt steel, and that steel is melted as fluid as water in an air-tight crucible, without oxidation or burning. Therefore if the iron pile in which the steel is inclosed is so formed, having end-pieces also, that the heat will allow it to settle together, so that the joints shall be substantially closed and shall keep out the air, the weld must be perfect—there can be no other result. At the Union Mills, in Pittsburgh, he had seen many examples of what any expert would call perfect welding of an iron-clad steel pile. When as much iron as steel was used, the iron became steely and the steel became more like wrought iron. Whether or not the steel in the bar rolled from an iron-clad pile is as strong as the same steel would be rolled normally without piling, is not satisfactorily determined, as far as he was aware.

DR. EGLESTON said that while there was no doubt that iron could be welded to steel, it was very doubtful whether it could be made commercially profitable to do it. The general experience with steel-headed rails has been, that while some of them were good, many of them separated, after a time, at the point of junction, from imperfect welding, and that taking the bad and the good together, it was less profitable to use the steel-headed than steel or even iron rails. The sample of rail shown certainly did not give promise that the rails would last very long in the track. Some of the steel-headed rails which were made in New York State, some years ago, had a wedge-shaped projection on the head which descended two or three centimeters into the web of the rail. The compression caused by the traffic forced the wedge down and opened out the web, so that the

head was sometimes loose for a considerable distance. No attempt, so far as he knew, had been made to ascertain the life of such rails. To express the life of a rail in years does not, of necessity, mean anything. The only way to express it is in traffic. It was found that *the very best* iron rails on the Northern Railway of France, would last for a traffic of 20,000,000 tons, while the ordinary ones did not exceed 14,000,000; that good steel rails had a uniform wear of one millimeter for 20,000,000 tons. It was shown that a steel rail of 30 kilos. was actually cheaper than an iron of 37 kilos. (see *Transactions*, vol. 3, p. 86), and would last fully ten times the traffic. There is, moreover, a point which has received but little attention, and that is the temperature at which the rail has been finished, which will influence the wear, and will frequently cause rails of the same manufacture to differ widely in their wear.

It does not seem possible that rails made of such heterogeneous materials as this one is shown by its fracture to be, could stand the wear of heavy traffic, for the rails, as has been shown on some of our prominent railroads, undergo a species of cold-rolling, which tends to tear welded surfaces apart, or, when they are made of soft steel, to deform the rails. He had seen such steel rails, still in the track, which after a traffic of 60,000,000 tons, and having but nearly three millimeters of wear, had the material of the rail so far rearranged by cold-rolling that the shape of the rail was entirely altered. No rail, composed of heterogeneous materials, could have stood such a change of shape. It would have been torn apart on the welding surfaces.

MR. COXE: We made some steel-headed rails in 1869 and 1870, with steel from the open-hearth furnace, cast into ingots and then hammered into slabs nine inches wide by two inches thick; these slabs were used for the heads of the rails, and welded on to iron piles at our mill. Most of these rails are still in our tracks, doing as good service as solid steel rails, and the only reason their manufacture was discontinued was our inability to make them any cheaper than a rail entirely of steel.

We have tried also the Booth steel-capped rails, made by clamping, when cold, a grooved head-bar of steel on to an iron stem and base; these gave good results, and would have done better had the steel been thicker; but this was also more costly than the solid Bessemer steel rail.

THE JENKS CORUNDUM MINE, MACON COUNTY, N. C.

BY ROSSITER W. RAYMOND, PH.D., NEW YORK CITY.

By the courtesy of Mr. Charles W. Jenks, of Boston, one of the owners of this interesting mine, I am enabled to lay before the Institute a *suite* of specimens, illustrating its peculiar formation and the paragenesis of mineral species which it presents; and a recent brief visit and rapid reconnoissance of the locality permit me to supplement this exhibition with a general description, based upon personal observation.

The mine is situated upon the Culsagee Fork of the Tennessee River, in Macon County, North Carolina, a few miles from Franklin. The most practicable route by which it can at present be visited is by wagon (or preferably on horseback) from Seneca City, S. C., a station on the Charlotte and Atlanta Air Line Railroad. The itinerary is as follows: to Walhalla,* nine miles; Walhalla to Highlands, twenty-nine miles; Highlands to the Culsagee, fourteen miles. By passing the night at or near Highlands, the mine can be reached and examined and the return to Highlands made on the second day; and the round trip, if performed in the saddle, can be completed in three days from Seneca City. It is not easy, with due regard to steed and rider, to do it in less time. The rough and precipitous mountain roads do not permit rapid travel; and the journey is at best a fatiguing one. But its slight hardships are overpaid by the stimulating effect of the climate and the great beauty of the scenery, both of which are destined to become more widely known than at present, to seekers for health and pleasure. Indeed the town of Highlands, already named, is almost entirely made up of northern immigrants, attracted by these advantages and by the inducement of cheap land for agriculture and stock-raising.

The Culsagee or Sugartown fork of the Tennessee traverses the belt of ancient crystalline schists on the west flank of the Blue Ridge. This belt abounds in valuable minerals. Among those of the non-metallic class may be mentioned graphite, soapstone, serpentine, asbes-

* A branch or cross railroad is in operation as far as Walhalla; but the single daily train runs at an hour not convenient for this expedition; and Seneca City is probably the best starting-point. At all events, I obtained there an excellent mount for the trip.

tos, and mica. The latter has been mined to considerable extent since 1870; and in some of the most productive localities* the curious discovery has been made that mica was mined here on a large scale not less than three centuries ago. The occurrence of mica utensils and ornaments in the mounds of the Northwest, and of mica cut into corresponding forms in the *débris* of these ancient workings, leaves, according to Prof. Kerr, no doubt that these workings were contemporary with the ancient copper-mining excavations of Lake Superior, and that their product was conveyed to the West by the operations of a prehistoric commerce, the existence of which is thus for the first time made known.

Mr. Jenks, in a paper before the Society of Arts of the Massachusetts Institute of Technology (December, 1876,) has suggested that the ancient Egyptians may have used corundum, on account of its superior hardness, to execute their inscriptions in porphyry. However this may be, there is, so far as I am aware, no evidence to show that the mineral and its uses were known to the former inhabitants of this country; nor do their mounds or other relics present specimens of workmanship requiring its employment. Its discovery in North Carolina is not, therefore, like the opening of the mica mines, merely the renewal of an earlier industry.

"This mountain tract of Laurentian rocks," says Prof. Kerr, "contains between 3000 and 4000 square miles of surface," the aggregate area of these rocks in the whole State being, according to the same authority, more than 20,000 square miles, or nearly half the territory of North Carolina. Along the middle of the belt now specially under consideration, a discontinuous line of outcrops appears at intervals from Cane Creek in Mitchell County through the intervening counties of North Carolina into Union County, Georgia, and perhaps still further south. These are called, in the State Geological Report, dikes of chrysolite or dunite.† Mr. Smith, who seems to have made the most extended examination, thus far, of the rocks of this belt, says the chrysolite bears decided marks of its eruptive origin, and cites in proof its crystalline structure, the disseminated octahedral crystals of magnetite and chrome iron which it contains, and the circumstance that although it is laminated, the strike of its laminæ seldom conforms to the strike of the inclosing rocks. "It cannot, therefore," he says, "be regarded as a regular

* Near Bakersville, for instance. See the Report of 1875, of Prof. W. C. Kerr, State Geologist, p. 300.

† See two papers by Rev. C. D. Smith, of Franklin, N. C., in the Appendix to the State Report of 1875.

interpolation, nor as an intercalation with the inclosing beds." It would be presumptuous to dispute, on the strength of a hasty examination of a single locality, an opinion so positively expressed; yet I should not be surprised if future careful study of all the localities should show these chrysolitic beds to be intercalated members of the formation in which they occur. Prof. Kerr, who speaks of them as dikes (and also as ledges and masses) without definitely pronouncing them to be eruptive, says they are more or less distinctly granular, and constitute in fact a chrysolitic sandstone of a yellowish to dull or dark olive-green color. The composition, as analyzed by Dr. Genth and Mr. Chatard, of specimens from Webster and Culsagee, N. C., is as follows:

	Genth (Webster).	Chatard (Culsagee)
Silica,	41 89	41 58
Alumina,	trace	0 14
Protoxide of iron,	7.39	7 49
Nickel oxide,	0 35	0.34
Magnesia,	49.13	49.28
Lime,	0.06	0.11
Loss by heating,	0 82	1.72
Chromic iron, etc.,	0 58	——
	100.22	100 66

Prof. Shepard, in the *American Journal of Science* for August, 1872, and Dr. Genth, in No. 1 of the Contributions from the Laboratory of the University of Pennsylvania, have discussed the mineral species associated with this chrysolite. They comprise corundum, chlorite (ripidolite, jefferisite), talc, chalcedony, hornblende (actinolite, asbestos), damourite, tourmaline, etc. In view of this paragenesis, it is noteworthy that Dr. Genth's analysis of the chrysolite shows so little alumina.

Whether the chrysolite be eruptive or not, the veins of chlorite which it contains are plainly of secondary origin, and the presence of crystallized alumina in them, as corundum, cannot well be explained as a result of fusion. Whether this alumina has been segregated from the constituents of the (now chrysolitic) wall-rock, or deposited as an original member of the series of strata, or brought in by percolation in solution, and precipitated by slow reactions, it is not easy to determine. The absence of potassa and soda from the analysis above quoted may have significance in this connection, since the exchange of these bases for alumina would leave them in solution, while the latter was precipitated as hydrate or (carbonic acid being present) as a basic carbonate.

Dr. Genth's conclusions from the chemical analysis of the associated minerals are stated as follows:

That, at the great period when the chromiferous chrysolite beds (in part subsequently altered into serpentine, etc.) were deposited, a large quantity of alumina was separated, which formed beds of corundum; that this corundum has subsequently been acted upon and thus been changed into various minerals, such as spinel, fibrolite, cyanite, and perhaps some varieties of felspar, also into tourmaline, damourite, chlorite, and margarite; that a part of the products of the alteration of corundum still exists in the form of large beds of mica (damourite) and chlorite slates or schists; and that another part has been further altered and converted into other minerals and rocks, such as pyrophyllite, paragonite, beauxite, lazulite, etc. Dr. Genth frankly declares his inability to explain the manner of these transformations, and says that we have at present no facts upon which a tenable theory could be built.

Taking the case of chlorite as an example, we find that some of the specimens from the Culsagee mine are apparently pseudomorphs after corundum, with central nuclei of the unaltered mineral. Others contain distinct, perfect and brilliant crystals of corundum. Dr. Genth is forced to conclude that in the latter case there has apparently been a recrystallization of corundrum, since, as he says, the crystals "appear to have formed *after* a great portion of the original corundum had changed into chlorite, as if there had been an excess of alumina ready for combination, which, not finding a supply of the requisite amount of silicic acid and basis, had again crystallized as corundum."

It is difficult to conceive of the precipitation and crystallization of anhydrous alumina. One would rather suppose the original deposit to have been a hydrate, such as diaspore or beauxite. Dr. T. Sterry Hunt says* of the latter mineral, "By intense heat this substance (beauxite) is converted into crystalline corundum resembling emery in its physical characters; but the presence of grains of corundum in the hydrated mineral seems to show that the transformation may take place at ordinary temperature." Dr. Genth thinks that all the specimens he has examined present no instance in which corundum could have been eliminated under such circumstances from the hydrate. On the contrary, he says, the presence of grains of corundum in the beauxite proves pretty conclusively that the latter is itself the result of hydration, and that the grains which

* Sill. Jour. (2) xxxii, 288, quoted by Dr. Genth.

have been found are remnants not yet converted. As to diaspore, it occurs, according to Prof. J. Lawrence Smith,* very intimately mixed with corundum; but Prof. Smith observed that all the corundum which he had examined contained from 0.68 to 3.74 per cent. of water, even when careful examination showed the absence of diaspore or other definite hydrate of alumina. Dr. Genth suggests that the distribution may be so minute that even microscopic observation would fail to show it. But in any case, the presence of water in the corundum might be regarded as a sign, either of commencing hydration or of closing dehydration. The problem is therefore still unsolved.

Dr. Genth's view, which is strongly supported by many of the specimens examined, rather favors the probability that large masses of pure corundum will be found by deeper working upon those veins or strata in which, near the surface, the corundum appears in nodules, inclosures and "remainders" only. In this view, the "veins" of chlorite, etc., are local alterations of the original belt of corundum, and possibly that belt might be exposed in a less extensively altered, or wholly unaltered, condition by explorations beneath the surface zone. On the other hand, a hypothesis which makes the corundum part of a fissure-filling, or the result of segregation from the country rock, gives no special ground, so far as I know, for the expectation of larger masses at greater depth.

So far as the chemical aspects of the problem are concerned, nothing can at present be added to the work of such experts as Genth, Hunt, and Lawrence Smith. In visiting the locality at Culsagee, I hoped to contribute to the discussion the result of a thorough examination of the outcrops and underground exposures of the different strata, embracing a study of the mineral paragenesis more satisfactory than those hitherto based upon hand-specimens. My intention was, however, completely frustrated by the present condition of the various openings, which it would have taken days of labor to clear of the rubbish impeding a careful examination. My own observations, therefore, leave me in uncertainty; but it is fair to say that they revealed nothing absolutely inconsistent with Dr. Genth's view. The beds of corundum which, according to that view, were originally separated before deposition, must have been several in number at this locality, and their deposition must have alternated pretty sharply with that of the chrysolite. But this fact, although it favors the notion of a subsequent segregation during metamorphosis, is not fatal to Dr. Genth's idea.

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* Sill. Jour. (2) x, 355 and xi, 53, quoted by Dr. Genth.

Mr. Jenks, whose superintendence of mining operations gave him opportunities for continuous study of the facts, says that sometimes the corundum and sometimes the chlorite appeared to be the matrix. The purest corundum, if I am not mistaken, was usually found in nuclei; but one of the veins, more frequently than the others, produced it in crystals. It would be unwarrantable assumption, in view of the circumstances, to attempt to add weight to either side of the theoretical discussion by the expression of a private judgment. It is only for the sake of frankness that I say, that while the particular evidences either favor or do not disprove Dr. Genth's theory, the general analogies of chemical geology seem to me to be the other way.

The dike or mass of chrysolite exposed at the Jenks mine constitutes a somewhat barren ridge, through which five parallel veins of chlorite run longitudinally, having a N.E. and S.W. course, and a dip of about 45° N.W. These veins vary from a few inches to several feet in width. Exact measurements were not practicable at the time of my visit, by reason of the *débris* which had caved into the accessible workings since active operations were suspended in 1873. The veins have been explored by several shafts, tunnels, and open cuts, besides a large amount of surface costeaning, executed for the purpose of ascertaining their number and position. The net result of these operations has been to demonstrate the existence of these five definite zones, in which corundum may be sought with confidence, and to show with a certain degree of probability, that the different veins have individual characteristics, one of them being apparently most likely to yield the gems, another the massive laminated, another the granular, opaque corundum, etc. The specimens exhibited herewith illustrate all these forms. Concerning the gems, I need hardly say that, being well aware of the repeated scrutiny to which every exposed part of the deposit has been subjected, not only by the workmen during the operation of the mine, but by numerous visitors since, I wasted no time in hunting on the ground for rubies or sapphires. But the specimens from this mine in many cabinets of the United States and Europe, from the great (opaque) ruby and sapphire crystal, weighing 312 pounds, now in the Shepard collection at Amherst College, to the superb crystals in the possession of a collector in Philadelphia, place it beyond doubt that the conditions controlling the formation of all the corundum gems have been present in various parts of this deposit. These gems are nine in number, including, besides the well-known ruby and sapphire, the

asteria, corundum emerald, corundum amethyst, girasol, chatoyant, and white or colorless sapphire. They differ from each other in coloring matter only, and from the common corundum in transparency and in the absence of cleavages which would interfere with the work of the lapidary. Strictly speaking, like all other gems, they possess crystalline cleavage. Even the diamond must be handled with much care in cutting, lest it be split and ruined. But the requisite degree of firmness may coexist with this property. The corundum gems heretofore known to commerce have always been found, in India, Ceylon, Bactria, the Ural, China, etc., as pebbles, rounded by long attrition in gravel. It has been of course inferred that they were crystals, torn from their original matrix, though, like the matrix of the diamond, this has been unknown. But it now seems more likely that these pebbles are the dense nuclei of larger crystalline masses, the more laminated, granular or cleavable parts of which have been rubbed away. At least, this is shown to be a possible origin by the discovery in the Jenks mine of transparent nodules of corundum in a matrix of the same material. Mr. Jenks says that specimens, more or less perfect, of all the nine varieties have been obtained from the mines.

On the other hand, if Dr. Genth's conclusions are accepted, it seems likely that the original bed of corundum would be opaque, and that the pure, transparent and variously colored gems would more probably result from recrystallizations, such as he suggests in the passage already quoted. This notion receives some confirmation from the circumstance reported to me, that one of the veins at Culsagee has produced more gems than the others, while it contains apparently fewer large masses.

Although the buildings and works at this mine are out of repair, it would not be difficult or expensive to resume operations. The value of the material in the arts, its superiority to ordinary emery, and the great demand for it, which could easily be revived and maintained, are matters beyond doubt. The facts were sufficiently established, a few years ago, when corundum from Chester County, Pa., was put into market. To the great regret of manufacturers, that source of supply became, at least temporarily, exhausted. I am informed that further explorations are making or to be made in that locality, with a view of discovering other pockets of mineral. Meanwhile, the product from there, together with that already taken from Culsagee, has "broken the ice;" and there is no doubt of a ready sale for the material, when delivered in suitable form and purity.

To secure these conditions, the product of the mine must be crushed and subjected to mechanical dressing, which will remove the associated minerals, except a small remainder of chromic iron. The proportion of this in the Culsagee corundum is fortunately trifling. I understand from Mr. Jenks that he has been able to dress his crude material up to 98 per cent. of pure alumina. If the works which I saw were able to do this, the problem of mechanical dressing cannot be very difficult. A small and rude stamp-mill with a few launders, comprised the whole apparatus. A supply of water both for power and for concentration was obtained by means of a ditch and reservoir. Perhaps operations on a larger scale would require steam-power. But the veins are narrow; mining is slow work; and even for a very profitable business, at the present price of corundum, a large product would not be required, even if it could be obtained. Mr. Jenks says that, without the aid of better machinery, and without improvements in transportation or favorable contracts with teamsters and railroads, based on a steady business, he can mine and dress a ton of prepared corundum for $40, send it to the railroad for $20, and deliver it in Philadelphia for $20 more, or $80 in all. Each of these items can, I believe, be greatly reduced. But the material is said to be easily worth in market $160 per ton, several tons having been sold at much higher figures. Without vouching for these estimates from personal knowledge, I feel safe in saying that they indicate a very favorable prospect for the enterprise. The only remaining question of vital importance is that of competition. The chrysolite belt is, as I have shown, an extensive one; and corundum has been found in it at numerous points. Whether at some future time, perhaps under the stimulus of the success achieved in a pioneer enterprise, rival mines, more favorably located as to transportation, may not be opened, it is of course impossible to predict. I have taken some pains to investigate as to the other localities heretofore reported, and have to acknowledge my obligations to Rev. C. D. Smith, of Franklin, N. C., the original discoverer and tracer of the chrysolite belt, for his cordial reply to the inquiries I addressed to him on the subject. Putting together what information could be obtained from all quarters, I am led to conclude that the Culsagee mine is the only one, so far, which has passed through the laborious and expensive stage of preliminary explorations, and definitely exposed the corundum-bearing deposits *in situ*. Moreover, those other localities hitherto discovered, which seem most promising as to mineral, are about as badly off as Culsagee with regard to transportation.

THIN PLATES OF METAL.

BY PROF. T. EGLESTON, PH.D., SCHOOL OF MINES, COLUMBIA COLLEGE, NEW YORK CITY.

THE importance of having perfectly pure metals has led me to present to the Institute a record of some of the trials that have been made to obtain these metals, and also to show one of the largest specimens of extremely thin metal plate which has ever been made.

The malleability of metals varies generally directly with their purity, and hence it is only with very pure metals that thin sheets can be obtained. The competition among different manufacturers has been so great at times as to lead to expensive and apparently useless experiments in obtaining in the first instance very thin sheets, and afterwards very large and thin sheets of metal, apparently with no other purpose than that of being able to say that they had produced the thinnest or the largest thin sheet that had ever been made. These experiments were at first confined exclusively to iron, their object being to show the great dexterity of manipulation, as well as the purity of the metal manufactured. They have since been extended to almost all metals by electrical action, and have gone far beyond the limits of what was possible with purely mechanical means.

The processes which have been used are rolling or hammering, electrical deposition in a vacuum, and lastly, electrical deposition on plates, easily soluble in acids. The first and last of these methods have been known and practiced for a very long time, having been used to make thin sheets of almost all the metals. The other is of quite recent application.

In the iron manufacture, the strife to produce these thin sheets was commenced in the year 1865, by the Sligo Iron Works writing to a firm in Birmingham, England, on a sheet of iron containing 270 square centimeters, and weighing only 4.469 grams. For some time this was considered to be the thinnest sheet iron that could be made. T. W. Booker & Co., of Cardiff, England, however, produced a few months afterward, a sheet of the same size, weighing only 4.015 grams. This was succeeded by one rolled by Neville and Everitt, of Llannelly, weighing only 3.174 grams. This was followed by one from Hallam, of Swansea, which was 283 square centimeters in size, and weighed but 2.979 grams; and this by one rolled by R. Wil-

liams & Co., of West Bromich, was 445 square centimeters, and weighed 3.173 grams. This was succeeded by the Hope Society of Tipton, which produced one measuring 1425 square centimeters, and weighing 11.529 grams. Finally the Upper Forest Tin Works near Swansea, produced a sheet measuring 155 square centimeters, and weighing 1.296 grams, which required 1888 together to make one decimeter in thickness. The thinnest tissue-paper is about four times as thick as this sheet of iron.

Recently Prof. Wright, of New Haven, has produced by means of electrical discharge, perfectly transparent films of iron and of magnetic oxide, but only over a very small surface.

No application has ever been found for such thin metal as this, but the skill necessary to produce it has led to great improvements in the machinery by which sheets of metal are rolled, and a decided advance in the quality of the iron.

The next step was to produce thin sheets of metals of all kinds, which led Prof. Wright* to invent a method by which he could produce films of almost any degree of transparency of any metal, depositing them with sufficient certainty and accuracy to make either transparent or opaque films. The opaque ones he has applied to the manufacture of speculum mirrors, depositing on glass films of gold, 0.000183 mm. thick, and of platinum, 0.000174 mm.

The special object, however, which I have in making this communication, is to show to the members of the Institute a film of transparent gold, which is one of the largest, and at the same time one of the thinnest ever produced. It is 5.7 millimeters square, and by transmitted light is perfectly transparent, and of a pale yellow color, except in spots where the film has been purposely doubled, where it is a very light green. This film was prepared by Mr. A. E. Outerbridge, of the United States Mint, in Philadelphia. It is estimated that its thickness is not more than 0.0001 mm. to 0.00015 mm, and it is 15,000 times thinner than ordinary printing-paper; the difficulty of asserting the thickness is owing to the fact that its weight does not appreciably affect the balance. It is not more than $\frac{1}{100}$ part of a single undulation of a green ray of light. It is so extremely transparent that it does not reflect the full golden color, partly because the gold still retains a trace of copper, which gives it a reddish tinge.

The method of preparing the plates was by depositing the gold

* Silliman's Journal, 3d Series, vol. 13 and 14.

from a galvanic battery on a sheet of thin copper, rolled down to a thickness of 0.005 mm. This was then cleaned and carefully burnished, and placed in the bath; when the gold had been deposited it was removed, and immersed in weak nitric acid for several days, dissolving out in this way almost the whole of the copper, while the gold floated on the surface; it was then floated upon glass, and placed within two plates. In order to ascertain the weights of previous films which were thicker, the copper was weighed before immersion in the battery, then taken out and reweighed, the difference giving the weight in gold, the calculation being based on the weight of a cubic decimeter of pure gold.

It is found that in this way gold may be spread over a space many times larger than the thinnest gold that can be prepared by beating. It is remarkable on this specimen, that in rolling the copper, imperfections in the rolls produced very slight irregularities in the copper film. These are all reproduced in the shape of fine striæ on the gold, probably owing to the fact that the copper was more compressed in the direction of these lines, and had thus been a better conductor, so that the gold was deposited there more rapidly than over the rest of the surface. When the plate is of appreciable thickness, these irregularities can be burnished out.

With a plate so thin as this, nothing can be done. It cannot be handled or touched, except floating on water, and then only with a fine camel's hair pencil. It is so perfectly transparent that the finest print can be seen through it without the least difficulty.

NOTE ON A DEPOSIT OF CADMIA IN A COKE FURNACE.

BY H. FIRMSTONE, LONGDALE, ALLEGHENY CO., VA.

Deposits of cadmia, or impure oxide of zinc, are of common occurrence in the upper parts of blast furnaces using ores containing zinc, and were very common in the charcoal furnaces of Virginia working the brown hematite ores found near the Cadent black slate.

These deposits were known to the old charcoal founders as "sulphur rings," and the characteristic green flame of burning zinc frequently to be seen at the tymp of furnaces working ores containing zinc, was held by them to indicate the presence of a "sulphur fall." When zinc is burning in considerable quantities at the tymp, the

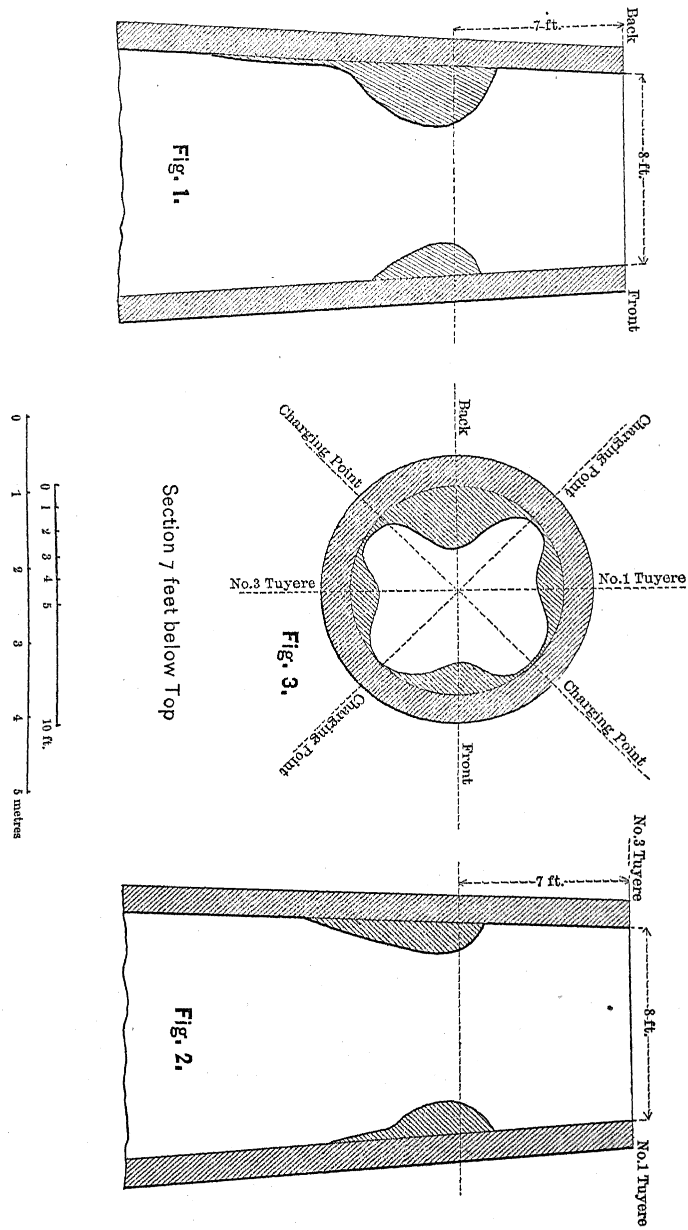
pieces of charcoal shovelled from the front are frequently covered with oxide of zinc, and while hot this oxide is of a yellow color, resembling somewhat flowers of sulphur, which tended, no doubt, to strengthen their belief that sulphur was present. These charcoal furnaces having, as was customary, very small tunnel-heads, frequently became much obstructed by the zinc deposits, and it was a common thing for them to be blown down for the purpose of "burning out the sulphur ring," it being supposed that the heat at the top while blowing down tended to remove the obstruction. How far this may have been the case is not known to the writer, but from the persistent manner in which they remain during blowing out in the top of a coke furnace with which he is familiar, he should judge that but little good was done by the operation.

Such deposits then being of common occurrence and so familiar to furnacemen, it will be the object of this paper merely to call attention to the large size and peculiar shape assumed by one of them in a coke blast furnace, the shape being determined by measurements taken after blowing out. Since it is very important for a proper discussion of all questions in relation to blast furnaces that all the data should be known, the dimensions, etc., of the furnace will first be given. The furnace in question is 60 feet high and 11 feet diameter at the largest place, which is about midway of the height; the tunnel-head is 8 feet diameter, and the furnace is filled by a modification of Coigut's charger (or double bell), as described by Mr. F. Firmstone (vol. ii, of *Transactions*, page 103), the gas being taken off by a central pipe. The hearth is 6 feet diameter, and has three tuyeres. About 3700 cubic feet of blast per minute is supplied to the furnace, at 3½ pounds pressure per square inch, the temperature of the blast being a full lead heat, rarely melting zinc. The coke used is New River coke, from West Virginia; the iron ore is brown hematite, obtained near the furnace in Allegheny County, Virginia, and contains about 46 per cent. of iron. The limestone is a pure carbonate of lime, supposed to correspond to the Pre-meridian limestone of Rogers's Pennsylvania survey. Twenty hundredweight and three-quarters (20¾cwt.) of coke are consumed in making a ton of gray-forge pig iron, the furnace producing about 185 tons of iron per week. The cubic contents of this furnace is about 3900 cubic feet. Several analyses of average samples of the iron ore have been made, but in no case was any zinc found. The same is true of the coke and limestone. The zinc undoubtedly comes from the iron ore, but must have been missed each time in sampling.

The deposit of cadmia, which it is the special object of this paper to describe, was formed during a fourteen months' blast, the furnace having been blown in September, 1876, and blown out November, 1877. In Fig. 1, a vertical section of this deposit is given, the section plane passing through the front and back of the furnace. In Fig. 2, a similar section is given, the plane passing through the two side tuyeres, Nos. 1 and 3. In Fig. 3, a horizontal section of the deposit or ring is shown, the section being taken at about the centre of the ring, or 7 feet from the top of the furnace. The materials to fill the furnace were dumped into the charging apparatus at four places, and the position of these four places on the circumference of the lining is marked on the horizontal section, as is also the position of the centres of the tuyeres below.

It is the peculiar form of this deposit, as exhibited by the horizontal section, that makes it interesting, and particularly so when compared with a horizontal section of a blast furnace described by Mr. F. Firmstone, and published in the *Transactions* (vol. iv, page 128), in which it was shown that the lining of the furnace was worn into six grooves at equidistant points on the circumference. In this case the materials had been dumped into the furnace (open top) at six equidistant points on the circumference, the grooves being midway between the points at which the stock was dumped in, as also midway between the centre lines of the tuyeres below. An examination of the horizontal section of the ring of cadmia (Fig. 3), shows it to have four prominent points directly over the tuyeres, and midway between the points at which the stock was dumped in, while directly under these last-named points there is but little deposit. In either case it seems most probable that the position of the tuyeres had nothing to do with the thing, but most likely it has been caused by the distribution of the materials. The shape of the ring of cadmia, with reference to the points at which the stock was dumped in, is just the reverse of that of the section of blast-furnace lining above referred to, as, indeed, the two cases of their formation are the reverse one of the other, one being a deposit (from the gases) on the lining, and the other a wearing away of the same; the projections in the one case and the grooves in the other, in all probability indicating the course of the ascending gases, for it seems plausible that we should look for the largest zinc deposit where the largest amount of gas passes, since the zinc incrustation is deposited from it. It is to be hoped that the presentation of this paper may bring to discussion some of these interesting, and to furnacemen, important questions.

When this paper was read at the last May meeting of the Institute, the writer was in possession of the measurement of but one of



these deposits, but since that time, the blowing out of the same furnace, after a little less than ten months' blast, has put him in

possession of the measurements of another deposit of cadmia, much larger than the one described, and which seems to follow the same law in its formation, although in not so marked a manner. This, however, would appear to be due to the much larger size of the deposit, the tunnel head of the furnace being much more nearly closed than in the first case. Considerably more ore was used during the ten months' blast than during the previous fourteen months' blast, the output of the furnace having been greatly increased, and hence the larger size of the deposit in a shorter time. This second deposit is exhibited by Figures 4, 5, and 6, in the same manner as the first.

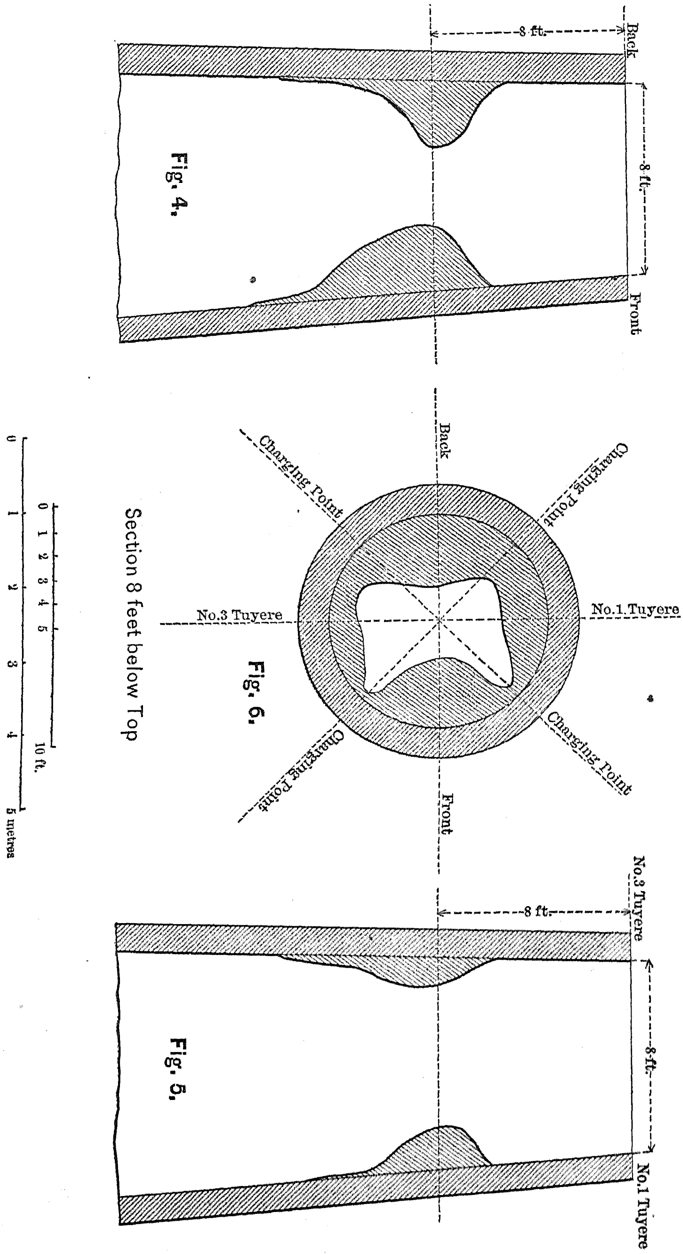
The first deposit on being removed from the furnace was found to weigh 4 tons 6 cwt.; the second, 12 tons 4 cwt. Both deposits were sold to a zinc company, I presume for conversion into spelter. An analysis of a piece from one of these deposits resulted as follows (by Dr. T. M. Drown):

Silica,	0.94
Iron (mostly metallic),	2 44
Oxide of lead,	0.90
Lime,	1.22
Oxide of zinc,	93.89
Carbon, by loss,	0.61
	100.00

It would seem that such deposits as this ought to greatly interfere with the regular descent of the charges, and thereby with the furnace carrying burden; but such was not the case here, the furnace having, during both blasts, carried a good burden up to the last, and having worked quite regularly, although in the last case the burden was not quite as heavy nor the work quite as regular as earlier in the blast. It will no doubt be necessary in the case of longer blasts being made at this furnace, to blow down occasionally, and cut out the deposit (a job that will not be easily accomplished, as the substance is very hard), and take it out of the top of the furnace, not allowing it to descend into the hearth.

The furnace in question has spells of several days' duration, when the hearth is filled with the flame of burning zinc, and scarcely anything but this characteristic green flame can be seen at the cinder notch and tapping-hole. Pieces of zinc are carried along on the surface of the cinder and iron while running, giving off the green flame as they pass, and leaving on the top of the cinder a deposit of oxide of zinc, yellow while the cinder is hot, but changing to a pure white when cold. One of the most peculiar things in this connec-

tion, and one which the writer never heard mentioned before, is the escaping of metallic zinc through the cracks of the hearth. This



takes place at the level of the tuyeres, and thence up, for a height of three or four feet. One hundred to one hundred and fifty pounds

might possibly be collected in this way during a year's run. In tearing out an old hearth many of the bricks are found permeated by globules of metallic zinc, the bricks being much shattered. Whether the bricks are first shattered, and the zinc then runs into the cracks, or whether the zinc is instrumental in the shattering, is an unsettled question, but it is quite certain that great difficulty has been experienced in getting the hearths to stand.

PROCEEDINGS

OF THE

LAKE GEORGE AND LAKE CHAMPLAIN MEETING,

OCTOBER, 1878.

THE members arrived at Ticonderoga, N. Y., at noon, Tuesday, October 15th, and were received by Mr. Cyrus Butler, Chairman of the Local Committee of Arrangements. During the afternoon the works of the American Graphite Company and the Horicon Iron Company were visited under the guidance of Mr. Butler, President of both companies, and of Mr. William Hooper, Superintendent. An excursion was then made, in carriages, to old Fort Ticonderoga, on Lake Champlain, after which the party were driven to the Rogers Rock Hotel, on Lake George.

The first session of the Institute was held in the parlor of the hotel on Tuesday evening. Mr. Eckley B. Coxe, President of the Institute, opened the proceedings with the following address:

I shall ask your attention, this evening, to a few reflections upon the subject of mining engineering as a profession in the United States, its past, its present, and its future, and its duties and opportunities with reference to the proper development of our great mineral resources.

Thirty years ago, there were scarcely any mining engineers in this country, and but few in England, and there were no institutions where young men could be educated for the profession in the English language. France and Germany have been able for about a century to boast of mining schools of a high character, that number among their graduates such men as Humboldt, Weisbach, Rittinger, Combes, and Gruner.

Of the few we then had worthy of the name of "mining engineer," some had studied in the continental academies, and others were graduates in the school of practical experience, and had learned their profession in mines and smelting-works. Their work consisted principally in making surveys and maps of mines and mining properties, geological reports, and analyses of ores; but the mining engineer whom we often meet with now, who has studied chemistry, physics, mineralogy, geology, mechanics, and drawing; who is more or less familiar with machinery and its construction, and with the practical management of mines or smelting-works, and who is an *expert* in some one branch of his profession, would have been very difficult if not impossible to find in the United States. The inducements held out to ambitious and talented young men to enter the

profession were, it must be admitted, very small. By a large portion of the community, a mining engineer was considered to be merely a bait with which unsophisticated capitalists were to be caught, and his opinions were considered to be worth so much a page and to favor any view which his employer wished to have advocated. He was seldom consulted as to the proper method of working a mine or running a furnace; he was rarely called upon by directors of works, except for extraordinary work, such as to give the direction in which a tunnel should be driven, or to analyze a new ore or limestone. There was no recognized standard in the profession, and there were great openings for unprincipled adventurers, of which many charlatans took advantage. Up to within fifteen years, mining engineers were too often regarded by those who had dealings with them with great distrust, and well-educated persons may yet be found who have no idea what a mining engineer is, nor what he is called upon to undertake.

This feeling is, however, wearing away. We now have mining engineers whose names are as well known abroad as at home; whose opinions are respected and paid for, although, I am proud to say, they cannot be bought. We have schools whose graduates are well qualified to enter the ranks of the profession when they have obtained the proper practical experience; and there are few parts of the country without mining engineers of established reputation, unless, in consequence of the peculiar condition of the locality, there is no need of their services.

Mining engineering is now recognized as an important profession, with its own periodicals and works of reference. The rapid development and the flourishing condition of this Institute, now numbering over seven hundred members, notwithstanding the almost unprecedented depression of all business connected with mining, shows how firmly the profession has taken root in the United States.

The causes of this change are very interesting, and to some of them I shall devote the greater part of the few moments that I shall detain you. No doubt the general development of the country, the advance in education, the better appreciation of the duties of the profession and of the aids that a mining engineer can give to industrial enterprises, have had their effects; but the most interesting and most efficient cause in bringing about the result is, I think, the great change in the problem which is now presented to owners of mineral properties, and which, to be successful, they must solve.

Thirty years ago, the effects of railroads had scarcely begun to be

felt. Mining (I here use mining in its widest sense—that is, as including all mining, metallurgical, and other enterprises which fall within the province of the "mining engineer," as that title is understood by this Institute)—mining, I repeat, was of a very different character from that with which we are familiar at the present day. In the first place, the want of means, or, what is the same thing, the great expense of transporting the ore, metal, or other product obtained, if it were very bulky or very heavy in proportion to its value, naturally limited the market, and consequently the output of the mines, iron furnaces, etc. If the market was not limited, the difficulty of obtaining more than a certain amount of charcoal where that fuel was used, or of bringing coal from a distance, prevented in another way the development of many industries. Sometimes the difficulty of transporting food, the thinness of population, or the lack of machine-shops for doing heavy work, and the lack of capital, prevented the building up of great industrial centres and the utilization of many now valuable deposits.

In consequence of these circumstances, mining industry was for the most part confined, except where specially rich or pure ores were found, to points possessing a local market, or where the works could be erected at a small expense, utilizing, perhaps, water-power, which is, although often limited and uncertain, economical for small establishments.

Little effort was made to economize fuel or ore; but everything was, as a rule, sacrificed to saving as much as possible in the original cost of the plant. The furnaces, buildings, machinery, etc., were, in consequence, generally of a very rude and defective construction, and were built and managed by men who had but little scientific knowledge of their business.

Under these circumstances, we can well see that there was but little place for the mining engineer. Works of such rude character, where cheapness of installation was generally the only point insisted upon by the proprietor, could be, and usually were, constructed by the mechanics of the neighborhood, under the direction of some practical miner or smelter. We must remember that the very fact of the difficulty and expense of transporting ore, fuel, etc., prevented, to a large extent, competition, and gave to the owner of the richest and purest ore-bed, or of the largest forest, or best water-power in a certain locality, a practical monopoly of the adjacent market; for, to be successful in business, all that was necessary for him was to be able to obtain his product at a less cost than his neighbors, whose

works were, of course, as rude, and whose method of production as expensive as his own. Land was cheap, the wants of the people few, and there had not been that development of the principle of association leading to the formation of joint-stock companies possessed of capital sufficient to put up expensive works, so arranged as to obtain, at a minimum cost, a large output of the best quality.

The actions of the miners and metallurgists were more like those of the hunters in South America, who, after killing the cattle on the pampas, take the hide only, leaving the meat upon the ground; as they can earn more money in the same space of time in that way, and do not care how much is wasted. "The cattle will last out our time!" they think.

With the introduction of railroads, supplemented by the canals already in operation, came the era of the cheap transportation of heavy and bulky articles, and with it the tendency to concentrate mining and metallurgical establishments in certain centres. It now became possible to work deposits of ore which formerly were so far from fuel as to be practically valueless, and to obtain ores and fuels of different grades and composition. The problem presented to the miner became much more complicated, and required ability and intelligence of a much higher order to solve satisfactorily.

In the good old time, as it was called, a landowner discovering a bed of iron ore on his property, apparently rich enough to work, would, if he were a prudent man, send a sample of it to the nearest forge and have it made into iron. If the experiment were not successful, the matter would be dropped; if successful, the owner, after having satisfied himself that he could obtain fuel enough, and that he could sell his product in his immediate neighborhood without fear of too great local competition, would employ one of the men from the forge where the experiment had been made to build one like it, and would then begin to make iron, limiting his production to the wants of the local market. There was comparatively little inducement to try to diminish the cost of production and to improve the product so long as iron from other districts did not come into his market.

Now the development of our railroad system has altered all this. Iron from one part of the country can be sent into almost any other, and the home market can only be held by the metal produced in any locality, so long as it can be sold at the same prices as that from a distance.

If a prudent person thought at the present day of investing his

money in the erection of an iron furnace, before beginning operations he would obtain samples of all the available ores, fuels, and limestones, and estimates of the prices at which they could be mined and delivered at the furnace. He would have them carefully analyzed, so as to be able to judge not only of their relative values, but also of the results which could be obtained by mixing them in various ways. He would endeavor to determine the cost of production and the selling price of the various kinds of iron he could make. He would thoroughly investigate the whole subject of the transportation, not only of the raw materials to his furnace, but also of his product to market, and of the food and other supplies needed for his works. He would also consider whether there were other centres of production which would compete with him for his market, and if so, on what terms. Another question not to be neglected would be, the number of workmen to be obtained in his immediate neighborhood, and whether the demand for laborers, occasioned by the erection of the new works, would compel their importation from other districts; and if so, how far this would increase the rate of wages he would be obliged to pay.

The next matter to be considered would be, how far economy in the cost of production could be realized in every department by the substitution of machinery for the work usually done by men.

In order to compete successfully, the works must be built in the best manner, care being taken not to increase the future cost of production by cutting down the estimates for the plant. To grasp this great problem, to thoroughly investigate it, and to draw the proper conclusion, is the best test of the true engineer; that is, of him who has mastered both the theory and the practice of his profession—of the man who neither neglects the theoretical speculations of the chemist, the physicist, the geologist, and the mechanical engineer, nor the facts and opinions of the practical men whose daily contact with the actual working of the furnaces enables them to make very valuable suggestions; for hints which at first appear of little importance sometimes lead to great results.

It is this change in the condition of the industries of the United States that has contributed in the greatest degree to build up our profession. The competition, not only of their immediate neighbors, but, owing to the improved methods of transportation, of the whole world, has forced upon the attention of owners of mines and furnaces the importance of having the best skilled scientific aid obtainable; for it now has become important to know not only what

advantage you may have over your competitors, and how you can best utilize them, but also those which they have over you. I take an example from my own experience. In pursuing the business of mining, shipping, and selling anthracite coal, our firm has expended a large amount of time, money, and labor in investigating the chemical composition and physical properties, not only of the coal we mine, but also of all others with which we come in contact in the market; and we think we know in what respects our coal is superior and in what inferior to that of our competitors as regards its appearance, its composition, its manner of burning, the nature and quality of ash, the location of its markets, etc. We do not imagine that we have got the best coal in the market for all purposes, but we try to know, as far as possible, the exact relative value of our coal for each purpose, and for each market. Knowing this, our business rule is to sell, if possible, our whole production, of each size, in that market, and for that purpose, to which it is best adapted. When this market is exhausted, we turn to that which then offers the greatest advantage. The importance of understanding exactly what you have to sell, and what the person to whom you are trying to sell really wants, is very great; for it is far more unfortunate to sell coal of a peculiar quality for a purpose to which it is not adapted, which naturally dissatisfies the purchaser and injures the reputation of the coal, than to miss making the sale.

We have found that the more educated scientific engineers we employ in our business, the more we try to keep ourselves *au courant* with what the profession is doing in the different civilized countries of the world, not only in our own branch—that is, coal—but in every other department of mining engineering, and the more carefully we follow the experiments and discoveries that are made by others, the more pecuniary profit we reap. For example, for the last eight or ten years, there has been a growing feeling, among those engaged in the mining of anthracite coal in Pennsylvania, that the old method of picking out all the slate by hand was too expensive, and for small coal almost impracticable, if the coal was to be well prepared, and that some mechanical method, such as jigging, would have to be adopted; and in consequence jigs have come into use.

Those first constructed were extremely expensive and complicated, and although, when working well (that is, when the machinery was not deranged), they made a very good separation, yet the difficulty of keeping them in good running order was so great that, notwith-

standing the fact that they had cost several thousand dollars apiece, they have been in most cases thrown out.

In the course of time, the attention of a number of inventors was directed to the problem, and simpler and less expensive, but equally effective, jigs were invented, until finally they were built of comparatively simple construction, for about four hundred dollars, delivered at the machine-shops. As the simpler jigs were brought into use, the more complicated and older ones were abandoned; but the amount of money expended in these experiments has been very great, certainly over one hundred thousand dollars. Our firm certainly has spent from five to ten thousand dollars during the last four or five years in purchasing and experimenting with them. About three months ago, I received the Freiberg *Jahrbuch* for 1878, and in it, to my astonishment, I found a complete set of drawings and a description of a coal-breaker now in operation in Saxony, which is very similar to those we have in Pennsylvania, and in which about four hundred tons of very impure coal are prepared for market daily. The coal, as it comes from the mine, contains about 25 per cent. of ash, and that shipped to market only 7 to 8 per cent., which is practically clean coal. They make six sizes, exclusive of the fine coal or dirt (as we call it) which is all saved.

These sizes correspond to our lump, egg, stove, chestnut, pea, and buckwheat. The breaker has been in successful operation for about three years, and all sizes except lump are prepared by jigging; and the product they obtain at an expense of about 5½ cents per ton is as clean as ours, and is obtained from much dirtier coal. The jig used is extremely simple; there are hardly any parts likely to get out of order; and it can be built and put into a breaker for very much less cost than the cheapest jigs in use at the anthracite collieries of Pennsylvania, probably little over one-half.

As soon as I had examined the article, I had one of these jigs built, and it has worked admirably from the very start, making a good separation and occupying little more than half the space of our old jigs, the cost being practically nothing for repairs.

The moral that I would draw is this: Had I, when the jigging question became an important one in our business, availed myself of the experience of European engineers, personally or by employing an expert, and made myself thoroughly familiar with the machines they were using, I could have begun where I left off (for I built a coal-jig of my own invention), and we could have had all our works completely supplied with simple, efficient jigs without having spent

as much money as we have done to find out that we were wrong. In other words, gentlemen, the day has gone by when any firm, company, or individual, having the management of large mines or metallurgical works, can afford to be without the services of a first-class mining engineer, not only in the selection of the location for the works, in laying them out and in erecting them, but also as adviser in their daily management. They want his opinion, not only as to what they should do, when they should do it, and how they should do it, but also as to what they should avoid. This is entirely outside of what many call the mercantile part of the business.

And now as to the future. Mining and metallurgy are, as we all know, very much depressed, not only in the United States, but throughout the civilized world, and the cause is, to a great extent, the manner in which works have been erected without the advice of mining engineers—in the highest sense of the word—of men who could and would have predicted the financial ruin of those companies or firms who, relying on the abnormal rise in iron or coal, have invested large sums of money, where everything that went into the works had to be paid for at a cost double and even triple what it would be now. A little consideration would have shown that the furnace was built or the mine opened at a point or under circumstances which were not adapted to the economical production of the iron ore or coal which the company proposed to sell, since there were already in existence many establishments able to furnish it, without loss, at a price which would be ruin to the former.

The mining industries of the country are undoubtedly suffering from the result of the abnormal advance in prices, particularly of iron and coal, in consequence of which furnaces were erected, mines opened, rolling-mills built, and those already in operation enlarged, without sufficient attention having been paid to the question just referred to.

It seems to me, however, that when the present crisis is past, our country will enter upon a new era as a producer of fuel and metals; that we shall supply not only our own market, but become a competitor for the markets of the world; that is, if the mining engineers are true to their profession and to themselves, and if those who own or control our great mineral and metallurgical resources will grasp the real situation of affairs, and avail themselves of and profit by the advice which the true mining engineer is able to give. My reasons for believing that this change is about to take place are:

1. The fact that the demand for coal and iron and other metals is

steadily increasing, not only in civilized, but also in half-civilized countries. Wood is becoming scarcer; steam is becoming daily more extended in its use; gas is being introduced everywhere; the uses of iron, and particularly of steel, are multiplying; the methods of working the metals are better understood; and there really seems no limit to the amount of iron that the world will consume.

2. The deposits of the Old World are becoming, not exhausted, but in many cases more expensive to work; the demand for purer ores requires them to be brought from a great distance; the coal necessary to supply the future demand must be obtained from ever-increasing depths, and the expense of getting it is increasing, partly on account of the great depth at which it is mined, partly on account of the desire to economize the valuable fuel (for which higher royalties are now paid), partly on account of the greater restrictions laid upon mining by the increasing strictness of the mining laws in Europe, partly on account of the greater demands of the working people, who now work in England fewer hours per week than formerly. This, diminishing the output per man and for each opening, increases the cost of production.

3. The greater and increasing cost of food abroad; for as not only a large part of the grain, but also of the cheese, pork, and beef consumed in Europe are imported from this country, the prices of these must be and must continue higher there.

4. The rate of interest, which was formerly at least fifty per cent. higher here than in England, has diminished very much; and although probably lower now than it will be hereafter, when trade revives, yet I do not think that the difference will be as great in the future as it has been in the past.

5. The tendency of wages to equalize themselves between this country and Europe. Now that the steamers carry passengers for about twenty dollars from England to the United States, it is becoming very common for miners and iron-workers to go back and forth as times are better in one country or the other. That this occurs, I know, for we have had several cases among our own men; and I feel satisfied that the practice is increasing, and that hereafter it will be carried to such an extent as to cause the rate of wages paid in the two countries to approach more nearly to one another.

6. The value of land and mineral properties is generally so much less in this country that a first-class establishment can often now be built up here at less cost than it could be abroad. In some cases—

though it may be well not to insist too much on that point—our taxes are less.

7. The great average intelligence of our foremen and workmen, and our labor-saving machinery.

To sum up briefly: Our comparatively undeveloped deposits, the increasing demand, our cheap food, the cheapness of the voyage across the Atlantic, the low cost of land, the intelligence of our workmen, and our labor-saving machinery ought to enable our industrial establishments, if properly located and conducted in the best manner—that is, with a view of obtaining the greatest product, of the best average quality, and at the lowest possible price—to compete successfully, before many years have rolled round, for the markets of the world in such things as iron, steel, and coal, as we already do in gold, silver, petroleum, wood, cotton, tobacco, etc. But, gentlemen, this is not like a prize in a lottery, to be won by chance or good luck; it must be worked for, striven after continuously, patiently, vigorously, and honestly. No want of success must discourage us or induce us to give up the contest.

The equation is not an impossible one; there is a solution; and the day will be a great one for the country when the solution shall have been found, as it must be.

Now, fellow-members of the American Institute of Mining Engineers, what is our share in the great work? how can we best aid in bringing about this so-much-to-be-desired result? By directing intelligently, vigorously, and honestly the active resources of the country into the proper channels; by preventing them from being turned away into the thousand ditches and swamps which lie upon all sides, ready to absorb here a little and there a little, and producing nothing but puddles of dirty water, which will only serve to foul the reputations of those who caused them. If we should be employed to superintend the opening of a mine, the erection of a furnace, the building of a rolling-mill, or in any other work connected with mining or metallurgy, let us not undertake it unless we feel that it is really within our branch of engineering, and that we are really expert in the matter, and capable, after having thoroughly studied the question, of carrying it through to a successful issue, provided nothing disastrous, which we could be expected to foresee, should occur. In the next place, let us satisfy ourselves that the enterprise has a *raison d'être;* that is, that if properly started and managed, it can defy any competition, which, as far as we can see, it is likely to be exposed to; and in considering this question, let us

not shut our eyes to any circumstance that may have an influence upon the question. If, after our examination, we feel sure that the risk is too great, we should state it to our employers openly, frankly, explicitly, without regard to the fact that we are sacrificing our own position. It is our duty, and it will pay us in the long run. If we merely have doubts, state them. Those whose money will be at stake are, of course, the proper persons to decide whether or not the risk shall be taken; but we should be sure, if they do not thoroughly understand the question, that it is not from any want of honest effort on our part to make them do so. Should they decide to go on, we should satisfy ourselves that sufficient funds will be forthcoming to complete the work properly, as originally laid out, and not begin with half what we know will be necessary to carry it to a successful completion.

And those who do not desire to compete for the markets of the world must remember that they will be crushed at home by those of their rivals who put themselves into condition to do so.

We should make a careful study of the whole question, so as to determine the proper location for the works, their size and general plan; we should familiarize ourselves with all that has been done, both in this country and abroad, in all those branches which have a bearing upon our problem; we should pay particular attention to everything that will tend to diminish the cost of production, to raise the quality of our product, and to make it uniform; for nothing adds so much to the reputation of an article, and enables it so well to hold possession of the market, as the fact that it is always the same; it will almost command better prices than a similar material, the quality of which, although better as an average, is variable.

We should arrange our works to use machinery as much as possible, instead of men; and it is a great advantage if you can have two classes of workmen only: intelligent, highly-skilled mechanics, who are well paid, and *worth their wages*, and ordinary laborers. It is well to avoid those who are neither fish nor flesh, neither good mechanics nor ordinary laborers, knowing too much to be willing to do ordinary work, and not enough to be employed as skilled mechanics. We should impress upon our employers the importance of thorough organization, perfect plant, regular work, uninterrupted by the breaking of cheap machinery or the incompetency of cheap men. We should endeavor to keep ourselves continuously *au courant* with what is going on, being ready to adopt every real improvement, but not every brilliant idea which has not been tested practically. And

when our work is finished, we should feel that we have done everything in our power to produce the most perfect work with the resources at our disposal, at the least possible cost to our employers.

If the works are managed by us when in operation, the same rules should guide our actions. We should remember that our first duty is to enable our employer to see and understand everything connected with the engineer's department as thoroughly as he does that which relates to his own. And if, after an honest, conscientious effort to bring the operation to a successful conclusion, it should fail —and fail it may, for the mining engineer cannot say with Richelieu, in the play, "There is no such word as fail!"—we should be able to write upon the ruined stack or the deserted shaft-house, "*Tout est perdu fors l'honneur.*"

The following members and associates were unanimously elected:*

MEMBERS.

Hermon B. Butler,	Ticonderoga, N. Y.
James R. Cameron,	Osceola, Clearfield Co., Pa.
William H. Case,	Port Henry, N. Y.
Dr. Charles B. Dudley,	Altoona, Pa.
Dr. Frederick Endlich,	Reading, Pa.
G. Clinton Gardner,	Altoona, Pa.
Charles L. Hammond,	Crown Point, N. Y.
William Hooper,	Ticonderoga, N. Y.
Joshua Hunt,	Catasauqua, Pa.
F. H. McDowell,	San Francisco, Cal.
John McFadyen,	Johnstown, Pa.
William Manthey,	South Pittsburgh, Tenn.
James Cosmo Newberry,	Melbourne, Australia.
Oliver S. Presbrey,	Port Henry, N. Y
John Provis,	Alamos, Sonora, Mexico.
George R. Sherman,	Port Henry, N. Y.
Dolphus Torrey,	Philadelphia.
Albert Tower,	Poughkeepsie, N. Y.
Lamartine C. Trent,	Belmont, Montana.

ASSOCIATES.

Edward B. Buckley,	Antwerp, Jefferson Co., N. Y.
Willard Parker Butler,	New York City.
T. Lloyd Haigh,	New York City.
Charles L. Palmer,	Port Henry, N. Y.
William H. Price,	Cleveland, Ohio.
W. G. Thomas, Jr.,	Brussels, Illinois.
John A. Walker,	Jersey City, N. J.
L. E. Warner,	Cincinnati, Ohio.

* In this list are included those elected at the third session of the meeting.

On the recommendation of the Council, the following associates were made members of the Institute:

Russell B. Harrison, J. B. Mackintosh, and H. B. Willard.

Dr. Charles B. Dudley, Chemist to the Pennsylvania Railroad, read two papers, The Chemical Composition and Physical Properties of Steel Rails, and Does the Wearing Power of Steel Rails Increase with the Hardness of the Steel? After remarks by many members on the importance of the subject presented by Dr. Dudley and the value of his communications, it was voted that the discussion of the papers presented be deferred until the next (February) meeting of the Institute.

Mr. E. B. Coxe made a short communication on a peculiar form of anthracite, of which specimens were exhibited.

Before adjournment of the session, the members present were each given a box of graded pencils by the Joseph Dixon Crucible Company, Mr. Orestes Cleveland, President, as a souvenir of their visit to the works of the American Graphite Company.

On Wednesday an excursion was made on the steamer Ganouskie, Captain Arnold Hulett, to the head of Lake George, stopping at the Hundred Island House for dinner. On returning, a special train was taken for Port Henry, stopping at Crown Point, where opportunity was given for inspection of the Crown Point Iron Company's furnaces.

The second session was held at the Opera House, Port Henry, on Wednesday evening, when the following papers were read:

The Wheeler Process of Welding Iron and Steel without the use of Fluxes, by Dolphus Torrey, of Philadelphia.

An Improved Tuyere and Pipe, by John M. Hartman, of Philadelphia.

New Determinations of the Coefficients of Friction of Lubricated Journals and of the Laws governing such Friction, by Prof. R. H. Thurston, Stevens Institute of Technology, Hoboken, N. J.

A Direct Process of Copper Smelting, by H. M. Howe, of Troy, N. Y.

On Thursday a special train on the Lake Champlain and Moriah Railroad was taken to Mineville, where the principal mines of Witherbees, Sherman & Co. and of the Port Henry Iron Ore Company were inspected. The party were here provided with lunch, hospitably provided by both companies. On returning to Port Henry, the Cheever ore bed, the ore wharves, the Cedar Point furnace, and the Port Henry furnaces were visited.

The third and concluding session was held on Thursday evening, at Port Henry, when the following papers were read:

The Butler Mine Fire Cut-off, by H. S. Drinker, of Philadelphia.

The Production of Charcoal for Iron Works, by John Birkinbine, of Philadelphia.

The following papers were read by title, owing to lack of time to read them in full:

The Water Supply at the Bessemer Steel Works of the Edgar Thomson Steel Company, by P. Barnes, of Springfield, Ill.

Shaft Surveying in the Brown Hematite Mines of Northampton County, Pa., by Ellis Clark, Jr., of Easton, Pa.

Experiments on the Removal of Carbon, Silicon, and Phosphorus from Pig Iron by Alkaline Carbonates, by Dr. Thomas M. Drown, Lafayette College, Easton, Pa.

The following resolutions, offered by Dr. R. W. Raymond, were voted unanimously:

Resolved, That the Secretary be instructed to convey the thanks of the Institute to the following gentlemen for the cordial courtesy and hospitality which have so greatly contributed to the comfort, pleasure, and profit of the Institute during the meeting now about to close, viz: Messrs Le Grand B. Cannon, President of the Champlain Transportation Company; Arnold Hulett, Captain of the steamer Ganouskie; Orestes Cleveland, President, and J A. Walker, Secretary, of the Joseph Dixon Crucible Company; Cyrus Butler, President, and William Hooper, Superintendent, of the American Graphite Company and Horicon Iron Company; Thomas Dixon, President, and T. Vorhees, Superintendent, of the Delaware and Hudson Canal Company; E Hedding, Superintendent of the Lake Champlain and Moriah Railroad; Walter Tefft, Superintendent of Witherbees, Sherman & Co.'s mines; G. G. Roe, Superintendent of the Port Henry Iron Company's mines; W H Case, Engineer of both companies; General Hammond, President, and Alvin L. Inman, Superintendent, of the Crown Point Iron Company; Captain McDonald, of the Crown Point Mines; T. F. Witherbee, Superintendent, I. Cary, Assistant Superintendent, of the Cedar Point Furnace; O. S. Presbury, Superintendent of Cheever Ore Beds; and W T. Foote, Superintendent of the Port Henry Furnaces, also to the Port Henry Iron Ore Company and Messrs. Witherbees, Sherman & Co.

Resolved, That special thanks be extended to Messrs Cyrus Butler, F. S. Witherbee, T. F. Witherbee, and A L Inman, the very efficient Local Committee, for the untiring zeal with which they have devoted themselves to the work of arrangement for the meeting, and that to the thanks of the Institute be added its congratulations upon the perfect success which has crowned their endeavors.

On Friday morning an excursion was made to the mines of the Crown Point Iron Company, under the guidance of Mr. A. L. Inman, General Manager, and Captain McDonald, Superintendent, and also to the ore separating works and forges of the same com-

pany. On returning to Crown Point, a number of the members availed themselves of the courtesy of the Delaware and Hudson Canal Company, and made an excursion to Montreal, the remainder returning home.

PAPERS

OF THE

LAKE GEORGE AND LAKE CHAMPLAIN MEETING,

OCTOBER, 1878.

NEW DETERMINATIONS OF THE COEFFICIENTS OF FRICTION OF LUBRICATED JOURNALS, AND ON THE LAWS GOVERNING SUCH FRICTION.

BY R. H. THURSTON, A.M., C.E., HOBOKEN, N. J.

Professor of Mechanical Engineering at the Stevens Institute of Technology.

THE writer became convinced, many years ago, that the generally accepted values of the coefficient of friction for lubricated surfaces were not applicable to such heavy machinery as he had been called upon, in the course of professional work, to design, to construct, and to operate. Experience frequently seemed to indicate, also, a wide departure, in such cases, from the accepted "laws of friction."

In the year 1869, or earlier, he invented an apparatus to redetermine these laws and these coefficients for a wide range of temperatures, pressures and velocities. The construction of the machine was delayed, however, some time, and no experiments were made for several years, when, finally, the construction of the apparatus was commenced in the workshops of the Stevens Institute of Technology, and experiments were begun in the Mechanical Laboratory organized by the writer in connection with the Department of Engineering.

These machines have now been in use about five years, and have furnished an immense amount of valuable information. Those now in use are of two styles: one designed and built by the Class of 1877, Figs. 1 and 2; the other a much larger machine, Figs. 3 and 4. The first has been already frequently described,* and a working drawing of the second has been also published.† For convenience, engravings of both are herewith reproduced.

In the "'77 machine," a journal, *F*, is made on the overhung extremity of a shaft carried in the two bearings, *B B'*. This journal is grasped by brass boxes which are carried in a pendulum, *H H*. They are forced against the journal by a screw which compresses a coiled spring with a pressure which is read off on the scale, *N M*. A weight at the lower end of the arm, at *I*, gives it the necessary resistance to deflection. The angle of deflection is measured on the

* In Scientific American, Railroad Gazette, Jour. Frank. Inst., Johnson's Cyclopædia, Knight's Mechanical Dictionary, etc.

† Railroad Gazette, Jan. 18th, 1878, p. 25.

arc, $P\ P'$, in pounds of frictional resistance at the surface of the journal, *i. e.*, in such units that the division of the figures indicated by the pointer, at any given angle of deflection, by the figure indicated by the other pointer on $N\ M$, which indicates the total pressure

FIG. 1.

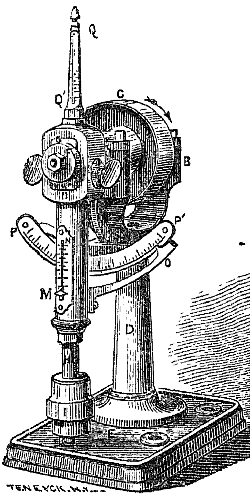
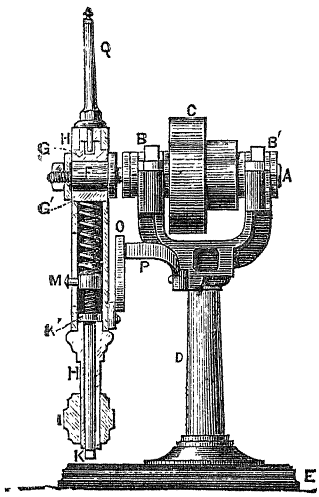


FIG. 2.



on the journal, gives the coefficient of friction. A thermometer, Q, gives the rise of temperature as the bearing warms up.

The machine is fitted for a wide range of pressures, as is seen on the index-plate, $M\ N$, on the pendulum, $H\ H$, where the large figures represent the total pressures on the journal, and those opposite the corresponding pressure per square inch.

The speed of the machine, when the belt is upon the largest pulley of the cone, C, should be that which will give the least speed of rubbing at the surface of the testing journal, which is to be usually adopted.

The figures on arc, $P\ P'$ traversed by the pointer, O, attached to the pendulum, are such that the quotient of the reading on the arc, $P\ P'$, by the total pressure read from the front of the pendulum at $M\ N$, gives the "coefficient of friction," *i. e.*, the proportion of that pressure which measures the resistance due to friction.

A printed table is furnished with each machine, giving these coefficients for a wide range of pressures and arc-readings.

To determine the lubricating quality, we remove the pendulum, $H\ H$, from testing journal, $G\ G'$, adjust the machine to run at the desired pressure, by turning the screw-head, K, projecting from the

lower end of the pendulum, until the index, *M*, above, shows the right pressure, and adjust it to run at the required speed by placing the belt on the right pulley, *C*.

We then throw out the bearings by means of the two little cams on the head of the pendulum, *H*, in the small machine, or by setting down the brass nut immediately under the head in the large machine; we next carefully slide the pendulum upon the testing journals, *G G′*, and see that no scratching of journal or brasses takes place.

Then we oil the journal through the oil-cups or the oil-holes, set the machine in motion, running it a moment until the oil is well distributed over the journal.

We next stop the machine; loosen the nut or the cams which confine the spring, and, when it is fairly in contact and bearing on the lower brass with full pressure, turn the brass nut or the cams fairly out of contact, so that the spring may not be jammed by their shaking back while working. The machine is again started, and run until the behavior of the oil is determined, keeping up a free feed throughout the experiment.

At intervals of one or more minutes, as may prove most satisfactory, we observe and record the temperature given by the thermometer, *Q Q′*, and the reading indicated on the arc, *P′*, of the machine, by the pointer, *O*. When both readings have ceased to vary, the experiment may be terminated.

The pendulum is then removed, after relieving the pressure of the spring, and the journal and brasses cleaned with exceedingly great care from every sign of grease; special care is taken to have no particle of lint on either surface, or any grease in the oil-cup or oil-passages.

A comparison of the results thus obtained with several oils will show their relative values as reducers of friction.

In each case we record in tables like the blanks, which are sent with the machine:

1. The pressure and speed of rubbing at each trial.

2. The observed temperature.

3. The readings of the arc of the machine, *i. e.*, the frictional resistance in pounds.

4. The calculated coefficients of friction.

We enter at the end of the trial the average and the minimum coefficients, and the total distance *rubbed over* by the bearing surfaces.

To determine the liability of the oil to gum, we allow the machine

to stand with the journal wet with oil, but with none feeding through the bearing, for 12 or 24 hours or more, as may be found necessary; then start up and run a few moments until the reading on the arc, $P P'$, having fallen to a minimum, begins to rise again; then stop *at once.* The minimum coefficients thus obtained from the several oils to be examined are then compared; that which gives the smallest figure will be least liable to gum during the period of time given to the test.

To determine durability, we proceed as in determining the friction, excepting that the lubricant should not be continuously supplied, but should be fed to the bearing, a small and definite portion at a time—say a drop for each two inches length of journal. Extreme care is taken that each portion actually reaches the journal and is not lost, either in the oil-hole or by being wiped off the journal, and that the portions applied are *exactly* equal.

When the friction, as shown by the pointer, O, has passed a minimum and begins to rise, the machine is carefully watched, and stopped either at the *instant* that the friction has reached double the minimum, or when the thermometer indicates 212° F.; or else another portion of the lubricant is then applied to the journal.

This operation is repeated until the duration of each trial becomes nearly the same; an average is then taken either of the time, of the number of revolutions, or of the distance rubbed over by the bearing, which average measures the durability of that lubricant.

We next carefully clean the testing-journal and proceed as before with the next oil to be tested.

In making comparisons we always test the standard, as well as the competing oils, on the same journal, and under *precisely* the same conditions.

An approximate value, by which to compare the oils, can be calculated, based on the assumption that they will have a money-value proportionate to their durability and to the *inverse* ratio of the value of the coefficient of friction.

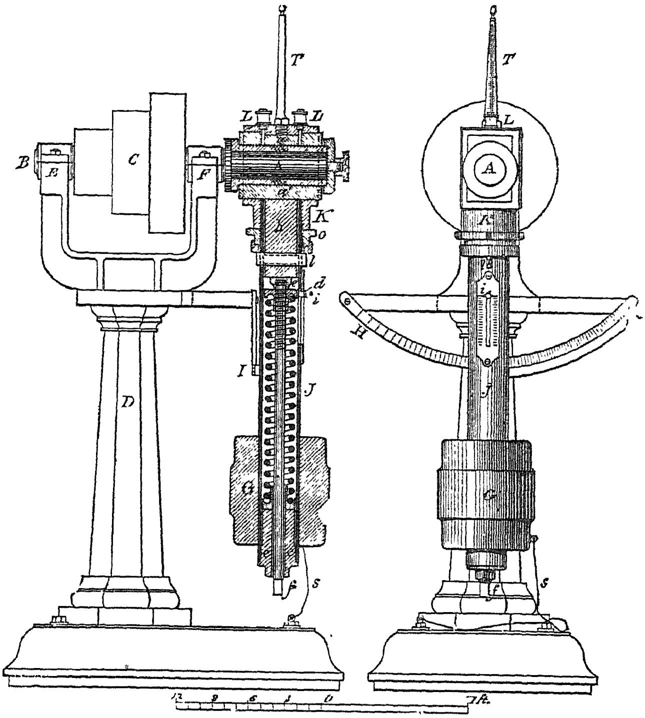
The larger machine is built to a scale about three times as great as that of the smaller, just described, and has a journal of standard car-axle size, $3\frac{1}{4}$ inches diameter and 7 inches long.

The speed is intended to be adjusted to speeds varying from that of a 26-inch engine truck wheel at 60 miles an hour, down to that of a 42-inch wheel running 15 miles an hour. The pressures are

adjustable from a few pounds total pressure up to 400 pounds per square inch, or a load of nearly 10,000 pounds on the journal.

Fig. 3 is a side elevation of the larger machine, with the journal and pendulum in section; and Fig. 4, a front elevation. It consists of a shaft, *A B*, which is driven by a cone pulley, *C*, the whole mounted on a cast-iron stand, *D*, terminating in a forked end at the top, with two bearings, *E* and *F*, in which the shaft runs. The

FIG. 3. FIG. 4.



shaft projects beyond the journal, *F*; and the projecting part, *A*, is provided with a sleeve or bushing, *m m*, the outside of which forms a journal on which the tests of oil are made. A pendulum, *A G*, is suspended from this journal with suitable bearings, *a a*, which work on the journal, *m m*. A heavy weight, *G*, is attached to the lower end of the pendulum. It is evident that the friction on the journal, *m m*, will have a tendency to move the pendulum in the direction of the revolution of the shaft, and that the greater the friction on the journal the farther will the pendulum swing. A scale or dial, *H I*, is attached to the stand, and the distance the pendulum

swings may be read off on this scale, which thus indicates the coefficient of friction of the lubricant on the journal. In order to get any desired pressure of the bearings on the journal, the pendulum is constructed as follows: A wrought-iron pipe, *J*, which is represented in Fig. 3 by solid black shading, is screwed into the head, *K*, which embraces the journal and holds the bearings, *a a*, in place. In this pipe a loose piece, *b*, is fitted, which bears against the under journal-bearing, *a'*. Into the lower end of the pipe a piece, *c c*, is screwed with a hole drilled in the centre through which a rod, *f*, passes, the upper end of which is screwed into a cap, *d*. Between this cap and the lower piece, *c c*, a spiral spring, shown in section in Fig. 3, is placed.

The upper end of the rod has a cap, *e*, in which it turns, and which bears against the piece, *b*, which in turn bears against the bearing, *a'*. If the rod is turned with a wrench applied to the square head at *f*, it is obvious that the cap, *d*, will be either drawn down on the spiral spring, which will thus be compressed, or it will be moved upward, and the spring will thus be released, according to the direction in which the rod is turned. If the spring is compressed, its lower end will bear against the under cap and on the piece, *c c*, by which the pressure will be transmitted to the pipe, *J*, and thence to the head, *K*, and from that on the upper journal-bearing, *a*; while at the same time the upper end of the spring bears against the cap, *d*, which, being screwed on the rod, *f*, transmits its pressure upward to the cap, *e*, and from that to the loose piece, *b*, and from that to the under journal-bearing, *a'*. It will thus be seen that any desired pressure, within the limits of the elasticity of the spiral spring, may be brought upon the journal and bearings by turning the rod, *f*. The piece, *b*, has a key, *l*, which passes through it and the pipe, *J*. This key bears against a nut, *o*, which is screwed on the pipe, its object being to provide a ready means of relieving the journal of pressure by simply turning the nut, *o*, when it is desired to do so. An index, *i*, is attached to the spiral spring, so as to show the position of the latter. The oil is fed to the journal by means of oil-cups, *L L*, on top of the head, *K*, and a thermometer, *t*, is attached between the two cups, and from it the rise in temperature is observed. A strap, *s*, is attached to the pendulum, to prevent it from being thrown beyond the limits provided for it.

The earliest determinations published, were given in the *Polytechnic Review*, March 3d, 1877. Later results were given in a lecture before the Master Car-builders' Association, in New York,

Dec. 20th, 1877;* and still later figures were given the American Railroad Master Mechanics' Association, at their annual convention held at Richmond, in the spring of 1878.†

In the course of this long series of experiments, extending over several years, and including work on all standard lubricating materials, and under pressures varying from 0 to 1000 pounds per square inch, at speeds of rubbing reaching 1200 feet per minute, and at all attainable temperatures, as well as with a great variety of material in journals and bearings, it was found that instead of being nearly constant in value, for even free lubrication, as assumed by engineers and physicists generally, the coefficient is variable in a marked degree by every change of pressure and of temperature; that it is affected by velocity-changes, and by the character of the metals of the journal and its bearings. A new journal also gives a coefficient which usually decreases, first rapidly, then more and more slowly, as it wears, while heating, and especially "cutting," causes an immediate and great increase of friction.

Friction with Varying Pressures.—An attempt was finally made to systematically determine the laws governing these variations of frictional resistance. A smooth journal, running in well-worn bearings, was lubricated with pure sperm oil, and the coefficients determined for a wide range of temperature, pressure, and speed of rubbing.

The following table contains data secured during one part of this investigation.

Studying table A, we see that the coefficient rapidly diminishes with increase of pressure, until a pressure of over 500 pounds per square inch is attained; the coefficient, after passing a pressure of probably 600 to 800 pounds per square inch, increases, and at 1000 pounds, becomes about equal to that obtained at 100 pounds. It will be remembered that 500 or 600 pounds pressure is usually considered to be a limit not to be exceeded in general practice in machine construction and operation.

* Railroad Gazette, January 18th, 1878, p. 23.

† Railroad Gazette, August 2d, 1878, p. 384.

TABLE A.—COEFFICIENTS OF FRICTION OF MOTION AND OF REST.—THURSTON.

(a) CAST IRON JOURNAL AND STEEL BEARINGS.

PRESSURES PER SQUARE INCH.	WINTER BLEACHED SPERM OIL.			WEST VIRGINIA OIL (MINERAL).			LARD OIL.			
Pounds.	At 150 ft. per minute. f.	At Starting. f'.	At Stopping. f''.	At 150 ft. per minute. f.	At Starting. f'.	At Stopping. f''.	At 150 ft. per minute. f.	At Starting. f'.	At Stopping f''.	
50	.0130	.070	.030	.0213	.110	.025	.0200	070	.010	Temperature in all cases, less than 115° F.
100	.0080	.135	.025	.0150	.135	.025	.0137	.110	.022	
250	.0050	.140	.040	.0090	.140	.026	.0085	.110	.016	
500	.0040	.150	.030	.0052	.150	.018	.0052	.100	.016	
750	.0043	.185	.030	.0050	.185	.014	.0066	.120	020	
1000	.0090	.180	.030	0100	.180	.017	.0125	120	.019	
(b) STEEL JOURNAL AND BRASS BEARINGS.										Ratio of $\frac{b}{a}$.
										SPERM. LARD.
500	.0030						.004			.75 80
1000	.0080						.009			.88 .90

Nevertheless, it is not uncommon to find as high pressure as 1000, or even 1200 pounds in the crank-pins of steam-engines. In such cases, however, the pins are almost invariably of steel, and the journals of good bronze—conditions which are less seldom met with elsewhere. There is also, in this case, as wherever a "reciprocating force" acts to move a piece, a condition which permits higher pressures to be successfully worked than can be reached elsewhere; the alternate application and relief of pressure, occurring between journal and bearing at each change of direction of the driving force, causes a release at such times, which permits the oil to find its way between the rubbing surfaces, and its expulsion is not then fully effected before the succeeding relief of pressure again permits its renewal. A somewhat similar action is consequent upon the rise and fall of a locomotive, or of a railroad car, in its springs, as it rapidly traverses even a smooth track.

Where, as in our testing-machine, under a flywheel-shaft, or in other machines, this relief cannot take place, the limit of pressure is earlier met.

Referring again to the last table, it is seen that between 100 and 750 pounds, the value of the coefficient may be obtained approximately by the expression,

$$f = \frac{a}{\sqrt{P}},$$

in which a is a constant quantity, and P is the pressure in pounds per square inch; for sperm oil, $a = 0.100$; for the best crude heavy mineral oil, $a = 0.150$; and for lard oil, $a = 0.125$.*

It will be presently seen that the law is modified by temperature and speed.

The following data were given by trials of two excellent kinds of grease and of sperm oil compared with them as standard:

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* Some of these facts and deductions were published originally in a paper prepared in the spring of the year 1878, and read at the St. Louis meeting of the American Association for Advancement of Science. This paper includes work done up to that time, supplemented by the more complete investigations since made. The research is still in progress, and determinations for other conditions will be published when obtained.

COEFFICIENTS OF FRICTION OF GREASES—THURSTON.

Steel Journal; Bronze Bearings; Velocity, 300 feet per minute.

LUBRICANTS.	PRESSURES, POUNDS PER SQUARE INCH.					
	100	200	300	400	500	Average
Sperm Oil	.0141	.0063	.0049	.0042	.0039	.0067
Grease, No 1...	.0249	.0146	.0125	.0105	.0114	.0140
" " 2...	.0188	.0198	.0160	.0146	.0175	.0173

Their relative average values, in reducing friction, stand, therefore: sperm, 100; No. 1, 44.8; No. 2, 37.7; which figures would also represent their relative money values, if estimated on that basis simply.

The method of variation with pressure, already noted, is here again illustrated, although the mathematical expression has a different set of constants, and the variation, at this speed, is more nearly as the inverse ratio of the cube root of the pressure.

Friction of Quiescence.—In Table A is presented a set of figures which are both new and important. In the columns headed "At 150 feet per minute," are given the coefficients of friction at the several pressures, as given when the rubbing surfaces are in motion at that relative velocity. These are the common and most usually required figures. We have given in the other columns, however, values which are seen at a glance to be immensely greater, and of which the values vary by an entirely different law. The first set, "At starting," are the well-understood coefficients of friction of rest, varying with the pressure and with the nature of the unguent from 0.07 to 0.18. These values have, I think, never been determined before in this way, and possess great importance, not simply intrinsically, but also as throwing some light upon the effect of motion upon the efficacy of lubrication. It is seen that they increase with the pressure, instead of diminishing, as do the coefficients of friction of motion; and that, at the highest pressures, their values become from ten to forty times the corresponding values of the latter. It is thus seen that, in the effort required to move heavy machinery, vastly greater force is demanded to overcome friction at the instant of starting, than after motion has once commenced. I presume that every experienced engineer or mechanic has known instances in which this difference has been so marked as to cause great difficulty in starting a machine, which, once in motion, moved with comparative ease.

TABLE B.—COEFFICIENTS OF FRICTION.—THURSTON.

New Journal of Steel; Bearings of Bronze; Velocity, Pressure and Temperature, Variable; Lubricant, Standard Sperm Oil.

SPEED PER MINUTE.	30 FEET PER MINUTE.					100 FEET PER MINUTE.					250 FEET PER MINUTE.		500 FEET PER MINUTE.		1200 FEET PER MINUTE.		
Pressure per sq. inch. Pounds.	200	150	100	50	4	200	150	100	50	4	200	100	200	100	200	150	100
Temp. Fahr.																	
175°	....	....	....	....	....	....	....	....	....	....	....	....	....	....	.0050	....	....
170	....	....	....	....	....	....	....	....	....	....	....	....	....	....	.0053	.0050	.0061
165	....	....	....	.0200	.125	....	....	....	....	....	....	....	....	....	.0056	.0050	.0061
160	....	....	....	.0175	.125	.0280	....	.0025	.0037	....	....	....	....	....	0056	.0053	.0061
155	....	....	....	.0137	.125	.0190	.0110	.0025	.0037	....	....	....	....	.0037	.0056	.0058	.0061
150	.0500	.0500	.0250	.0125	.125	.0140	.0074	.0025	.0037	.0630	.0047	.0028	.0053	.0037	.0060	.0058	.0061
145	....	....	....	.0112	.125	.0110	.0058	.0025	.0037	.0630	....	....	....	....	.0060	.0058	0070
140	.0250	.0330	.0110	.0087	.125	.0100	.0050	.0025	.0037	.0630	.0047	.0030	.0053	.0037	.0062	.0058	.0070
130	.0160	.0200	.0044	.0075	.125	.0087	.0041	.0019	.0037	.0630	.0047	.0030	.0053	.0037	.0065	.0062	.0075
120	.0110	.0110	.0044	.0075	.125	.0056	.0035	0019	.0037	.0630	.0047	.0037	.0056	.0037	.0069	.0067	.0080
115	....	....	....	....	....	....	....	....	....	....	.0048	....	.0060	.0014	....	....	....
110	.0100	.0033	.0037	.0062	.094	.0044	.0033	.0019	0037	.0630	.0050	.0044	.0062	.0050	.0075	.0075	.0087
100	.0075	.0028	.0031	.0056	.094	.0040	.0033	.0019	.0037	.0630	.0056	.0045	.0065	.0061	.0081	.0083	.0094
95	.0060	.0025	.0031	.0050	.094	.0010	.0033	.0019	.0037	.0630	.0062	.0050	.0072	.0061	.0087	.0092	.0125
90	.0056	.0025	.0031	.0037	.094	.0040	.0033	.0019	.0037	.0630	.0070	.0052	.0075	.0061	.0100	.0170	.0150

The method of variation of the coefficient for rest, is seen by reference to the table, to be such that their numerical values may be approximately estimated, for the cases here considered, by the formula,

$$f = 0.a' \sqrt[3]{P}$$

in which $a' = 0.02$ for sperm and heavy mineral oil, and $a' = 0.15$ for lard oil.*

The figures in the columns headed "At stopping," were given while the machine was rapidly coming to a stop, after the driving belt had been shifted to the loose pulley. They are, as would be expected, intermediate in value between the other figures, and have, apparently, no practical importance. They may be taken as constant at all pressures.

Friction with Varying Velocity—Thurston.—Referring to the accompanying Table B, in which the effects of varying velocities, as well as of coincident variation of pressure and of temperature, are exhibited, as given by experiment in the Mechanical Laboratory of the Stevens Institute of Technology, it is readily seen that the change in value of the coefficient of friction with change of velocity is not great for machinery in which that velocity remains within usual limits, and at the usual temperature of a cool and properly working journal. The effect of change of velocity varies, as is here shown, with change of temperature and of pressure.

For cool journals in good condition, lubricated with good sperm oil, and between the limits of 100 and 1200 feet per minute, these values may be taken as varying approximately as the fifth root of the speed of rubbing, *i. e.*,

$$f = a \sqrt[5]{V}.$$

At a constant pressure of, say, 200 pounds, we may call $a = 0.0015$.

Friction with Pressure and Velocity Varying Simultaneously.—We may now readily construct an equation which shall give values of the coefficient of friction for good journals lubricated with sperm

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* See paper, by the author, in Proceedings of American Association for Advancement of Science, St. Louis meeting, 1878.

oil, and for all ordinary variations both of pressure and velocity, since $f \propto \sqrt[5]{V}$ and $f \propto \sqrt[2]{P}$, we have approximately,

$$f = a\frac{\sqrt[5]{V}}{\sqrt[2]{P}}$$

in which we may take $a = 0.02$ to 0.03 for general use.*

The value of a for other conditions, and with other materials, may be determined by a comparison, by means of the testing machine, of those materials under the assumed conditions, with those obtained with the standard oil under standard conditions, as here given.

Friction with Varying Pressures, Speeds, and Temperatures.—We have here some exceedingly interesting data. The following were obtained by heating the bearing by its own friction to a maximum, 170° Fahr., well within that liable to produce alterations of the oil, and noting the temperatures while cooling. It should be remembered that no temperature-reading can be taken as more than approximate.

FRICTION AND TEMPERATURE.—THURSTON.

Steel Journal; Bronze Bearings; Lubricant, Sperm Oil; Velocity, 30 feet per minute.

Pressure per square inch.	Temperature, Fahr.	Coefficient of friction. f.	Pressure per square inch.	Temperature, Fahr.	Coefficient of friction. f.
200	150	0.0500	150	110	0.0035
200	140	0.0250	100	110	0.0025
200	130	0.0160	50	110	0 0035
200	120	0.0110	4	110	0.0500
200	110	0.0100	....	....	....
200	100	0.0075	200	90	0.0040
200	95	0.0060	150	90	0.0025
200	90	0 0056	100	90	0.0025
....	....	....	50	90	0.0035
....	....	....	4	90	0.0400

The figures just given would indicate that the sperm oil used in this instance and under these conditions, including that of excep-

* See paper, by the author, "On Friction Measurements," etc., Proceedings of American Association for Advancement of Science, 1878.

tionally low speed, works best at lowest temperatures, and that a heating journal gives rapidly increasing friction and rapidly increasing danger. At usual temperatures, 90° to 110° Fahr., the best pressure seems to have been from 100 to 150 pounds on the square inch.

It would, however, be wrong to predicate general conclusions on such limited data, and the study of Table B. is exceedingly interesting and instructive.

There are there given coefficients of friction for temperatures from 90° to 150° Fahr., for pressures up to 200 pounds per square inch, and for velocities of rubbing up to 1200 feet per minute. This table is, in fact, a compendium of data for all ordinary conditions of the working of light and of moderately heavy machinery. We find there some exceedingly valuable and very curious facts, bearing directly on our every-day work.

We have seen that at the low speed of thirty feet per minute, the coefficient increases rapidly with increase of temperatnre, and that at 200 pounds pressure an increase of 50° Fahr. may increase its value to nearly ten times the minimum, the rate of increase rapidly rising as pressures are greater.

We now find, at speeds of 100 feet per minute, that the friction does not vary between 90° and 150° F at pressures below 50 pounds per square inch, but that it rises nearly 300 per cent. at a pressure of 200 pounds, over 100 per cent. at 150 pounds, and 33 per cent. at 100 pounds.

At speeds exceeding 100 feet per minute, heating the journal within this range of temperature *decreases* the resistance due to friction, rapidly at first, then slowly and gradually a temperature is approached at which increase takes place and progresses at a rapidly accelerating rate. It is seen that this change of law takes place at a temperature of 120° F. and upward; at all higher speeds the decrease continues until temperatures are attained exceeding those usually permitted in machinery, and very commonly not far from 150° F., and sometimes up to 180°, or probably even higher. I have found the decrease, at 1200 feet per minute, to continue up to 175° F., at which the value at 200 pounds pressure was, in the cases determined, 0.0050. The limit of decrease is reached, under 100 pounds pressure, at 150° F., when running at this high speed.

At 200 pounds pressure the *temperature of minimum friction*, for conditions here illustrated, seems to be, in Fahrenheit degrees, about

$$t = 15 \sqrt[3]{V}.$$

On either side this point on the thermometric scale it may be assumed, for a narrow range, to vary as the temperature departs from that point, directly or inversely, as the case may be, as the temperature. The fact is, however, that this coefficient of minimum friction is found, usually, over quite a wide range of temperature.

Again, studying in this most instructive of our tables the method of variation with pressure at higher temperatures, we find the effect of change of pressure to be much more marked at the higher temperatures with low speeds, and, singularly enough, as when studying the effect of variation of friction with change of temperature at a standard pressure, as affected by the variation of speed, we here find a change of law for the higher speeds.

At a velocity of 1200 feet per minute the coefficient remains practically uniform, with varying pressure at 150° F., while below that temperature the friction-coefficient diminishes with increasing pressure. At velocities of rubbing of 250 to 500 feet per minute the temperature of the constant coefficient is about 100°; at 100 feet this peculiar condition is seen at about 120°, when extreme pressures (4 pounds and 200) are compared, but the value is seen to be a little over one-half as much at 50 and 150 pounds, and to become a minimum—0.0019—at 100 pounds pressure; a similar behavior is noted at the lowest speed observed—30 feet—at about 125° F., and the same fall to a minimum at the intermediate pressure.

It would seem that, at all times, there is a tendency to acceleration of outflow from the journal, with increasing fluidity due to increasing temperature, which tends to cause an increase of friction, while the effort of capillarity to resist this outflow seems effectively aided by increasing the velocity of rubbing.

A *balance* between these opposite influences is seen to take place at the slowest speed, when the pressure is somewhere below 4 pounds per square inch; this occurs at a speed of 100 feet per minute, at a pressure of 50 pounds; at 250 feet when the pressure becomes about 150 pounds; probably it happens at the speed of 500, at somewhere about the same point; and at 1200 feet per minute the benefit of increased speed is sufficient to produce this balance, when the pressure exceeds 200 pounds per square inch.

These data would seem to give very useful information relating to the method of action of lubricating materials.

Finally, it has now become evident that such a series of comparisons as I have here made, are needed in every case in which the real value and the extent and the conditions of application of any single oil

or other unguent are to be learned. Such a systematic examination reveals precisely the conditions which the lubricant best meets, and tells, with a certainty, at what pressure and at what speed it does its best work.

Conversely, the speed, the pressure, and other conditions of working being known, a reference to a set of such determinations for various lubricants being made, it is easy to ascertain at once which is best adapted to the work in view.

Thus, having had occasion to determine the value of the friction-coefficients of a material having a very high reputation as a "cylinder-oil"—*i. e.*, an oil for use in the cylinders of steam-engines,—it was found that its distinguishing peculiarity, as compared with oils not specially adapted for such purpose, was a continually diminishing coefficient, quite up to the limit of temperature of locomotive steam-pressure.

Any new lubricant should always have its true value and best adaptations thus determined. I have presented the figures for a fine steel journal, running in good bronze bearings and lubricated with sperm oil, not simply as an illustration, but principally as representative of the best set of conditions for use as a *standard* in making comparisons of other unguents of less value, or less known, or under less favorable conditions.

CONCLUSIONS.

Studying the facts here laid out, and the data acquired by many hundreds of other experiments made on one or the other of these last-described machines for testing lubricants, we may now recapitulate facts and figures for ordinary use in machine-design and in estimating losses of power by friction.

1. The great cause of variation with well-cared-for journals is alteration of pressure, and it is seen that the higher pressures, within the range of these experiments, give the lowest percentages of loss of power by friction.

2. The value of the coefficient is greatly modified by the state of the rubbing surfaces; and the necessity of keeping journals in perfect order cannot be too strongly insisted upon. A single scratch has its effect in wasting power. A journal should have its surface as smooth and as absolutely uniform as a mirror. Every well-kept journal has such a surface.

3. For general purposes, the value of the coefficient may be ob-

tained pretty closely by dividing 0.08 to 0.10 by the square-root of the pressure in pounds per square inch,—*i. e.*, $f = \frac{0.10}{\sqrt{p}}$—in which case it is given as that fraction of the total pressure which measures the resistance due to friction.

4. The coefficient for rest or starting may similarly be taken as about 0.02 the cube-root of the pressure—*i. e.*, $f' = 0.02 \sqrt[3]{p}$—for sperm and for crude mineral oil of good body, and 0.015 the cube-root—*i. e.*, $f' = 0.015 \sqrt[3]{p}$ for lard oil. For closer estimates, the tables just given can be referred to directly. It would seem that each material has its own coefficient and its own appropriate exponent in the expression, $f = c\,P^x$.

5. The coefficient for the instant of coming to rest, under conditions such as are here given, is nearly constant and may be taken at 0.03.

6.. The resistance due to friction varies with velocity, decreasing with increasing velocity, rapidly at very low speeds, as from 1 to 10 feet per second,* and slowly as higher speeds are reached, until the law changes, and increase, at ordinary temperatures, takes place and at a very low rate throughout the whole range of usual velocities of rubbing in machinery.

The value may be taken for use in machine-design and mill-work, and at a pressure of 200 pounds per inch, as $f = 0.0015 \sqrt[5]{V}$.

7. With pressure and velocity varying we may take, roughly, $f = 0.02 \frac{\sqrt[5]{V}}{\sqrt[2]{P}}$ to $f = 0.03 \frac{\sqrt[5]{V}}{\sqrt[2]{P}}$.

8. That the effect of heating journals under conditions here illustrated is to *increase* the friction in proportion to the square of the increase of temperature above, say, 90° or 100° F., at a speed as low as 30 to 100 feet per minute, while at higher speeds the opposite effect is produced and the coefficient *decreases* more nearly as the square-root of the rise of temperature.

Under all conditions commonly met with by the maker of machines, the latter is the method of change.

9. The temperature of minimum friction, under the conditions of these experiments, is about $t = 15 \sqrt[3]{V}$ for a pressure of about 200 pounds per square inch.

10. The endurance of any lubricant should be determined by actually wearing it out upon a good journal under the pressures and velocities proposed for its use.

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* We deduce this conclusion from the experiments of Professors Jenkin, Ewing and Kimball, at speeds below any given in this paper.

The economy with which it can be used will also be dependent upon its natural method and rate of flow, and upon its capillary qualities, as well as upon its intrinsic wearing-power, and the method adopted in feeding it. Greases, therefore, are usually more economical than oil, *even if having a less wearing capacity.*

11. The only method of learning the true value of a lubricant and its applicability in the arts, is to place it under test, determining its friction-reducing power and its other valuable qualities, not only at a standard pressure and velocity and at ordinary temperatures, but measuring its friction and endurances as affected by changing temperature, speeds and pressures throughout the whole range of usual practice.

12. The true value of an oil to the consumer is not proportional simply to its friction-reducing power and endurance under the conditions of his work; but its value to him is measured by the difference in value of power expended, using different lubricants, less the difference in total cost of oil or grease used; but, for commercial purposes, no better method of grading prices seems practicable than that which makes their market value proportional to their endurance divided by their coefficients of friction.

The consumer will usually find it economical to use that lubricant which is shown to be the best for his special case, without respect to price, and will often find the real economy in using the better material, sufficient to repay the excess in total cost very many times over. He cannot afford to accept low grade unguents, even without charge.

13. To secure maximum economy, the journal should be subjected to a pressure determinable by either Rankine's or Thurston's formula; the most efficient materials should be chosen for the rubbing surfaces; they should be reduced to the most perfect state of smoothness and perfection in form and fit; a lubricant should be chosen which is best adapted for use under the precise conditions assumed; the lubricant should be applied precisely as needed, and by a method perfectly adapted to the special unguent chosen.

The semifluid lubricants, if equally good reducers of friction, are usually most economical, in consequence of their peculiar self-regulating flow, as the rubbing parts warm or cool while working.

Where heating is not to be anticipated, maximum economy is obtained by the minimum rate of supply allowable only if that supply be maintained with absolute uniformity.

STEVENS INSTITUTE OF TECHNOLOGY,
HOBOKEN, N. J., September, 1878.

SHAFT SURVEYING IN THE BROWN HEMATITE MINES OF NORTHAMPTON COUNTY, PENNSYLVANIA.

BY ELLIS CLARK, JR., GLENDON IRON WORKS, EASTON, PA.

THE greater portion of the brown ore in the vicinity of Easton, along the north slope of the Lehigh Mountain or Durham range of hills, is obtained from mines instead of from open cuts or quarries, as is the case in the ore-beds in the western part of Northampton County, in Lehigh County, and in Berks County.

The reasons for this method of procedure are various, and the principal causes of the shaft and regular mine system of working being adopted instead of the open air cuts, are: First, the large amount of stripping that would be necessary, as the richer and more valuable portions of the ore do not appear to be found as near the surface as those in other portions of the Kittatinny valley; second, the occurrence of the ore in regular beds, interstratified with the clay, and, to a less extent, in pockets or erratic deposits, as is the case in the mines of Lehigh County; and third, on account of many of the mines being situated at the immediate base of the Lehigh Mountain, where the surface water would be in such large quantities as to be with difficulty handled, and where the subsequent caving in of the sides of the open cut would be almost certain to expose the gneiss rock, which is water-bearing, the large quantities of water coming from which would probably drown out the ore-bed. Shafts, and a regular system of gangways and mining are therefore required by the configuration of the surface, the depth of the deposits and their geological structure.

The ordinary size of the shafts is about four feet square, but where an extensive plant of pumping machinery is required the size may be increased to eight feet by six, with two compartments for the hoisting and pump-shaft respectively. A man-way, consisting of ladders with rests at intervals of twenty or thirty feet, is comprised in the pump-shaft, the wire rope being used for purposes of ascent and descent only in exceptional cases, on account of accidents which

have happened through the breaking of the rope, and precipitating the bucket with its occupant to the bottom.

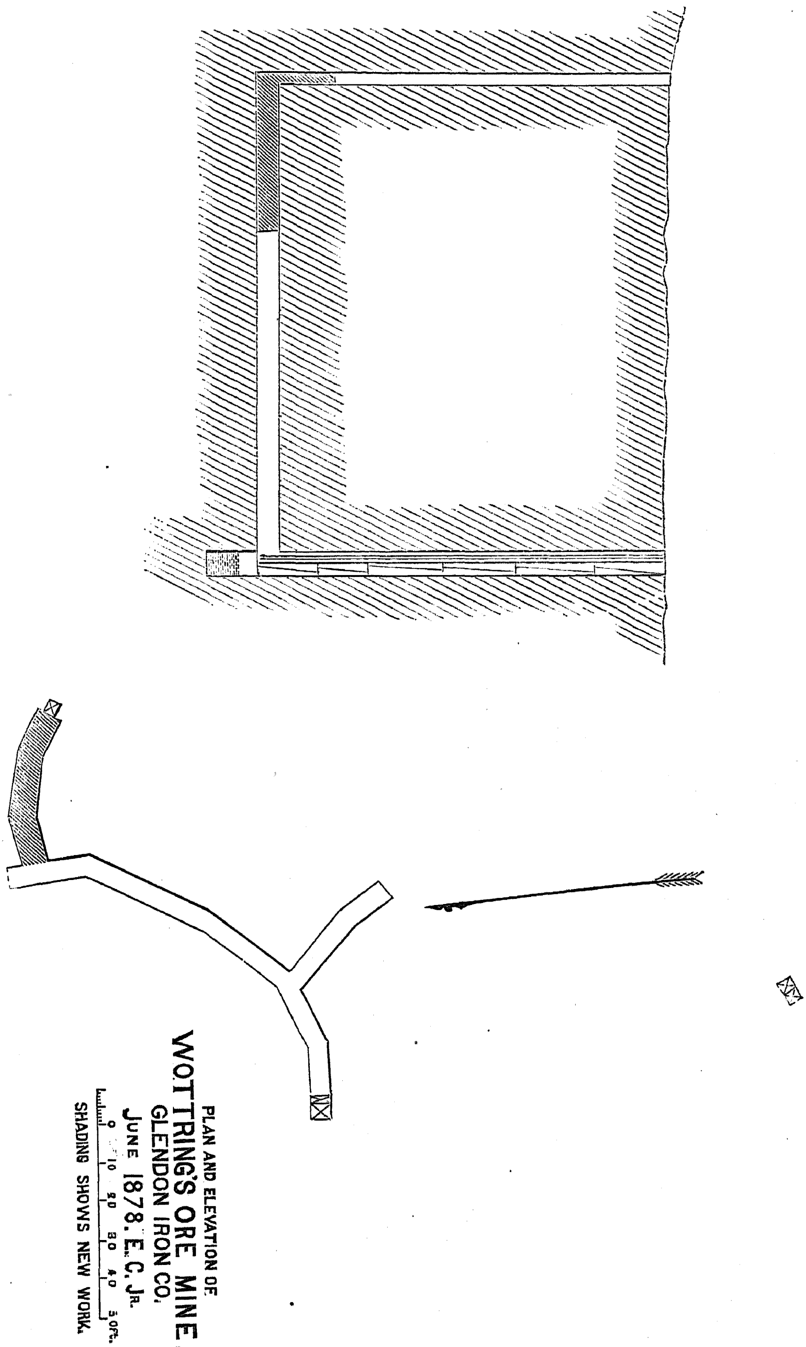
The principal difficulty in the survey of these mines consists in the trouble experienced in connecting the underground survey with the surface survey, both on account of the small size of the shafts and also the gradual movement of the ground outwards from the mountain, which, in a few months, may push a shaft considerably out of the perpendicular. The depth of the shafts varies between seventy-five and two hundred feet, so that a very small movement will suffice to throw them from their perpendicular position.

The method adopted by the writer in the survey of these shafts has been to establish a line on the surface, and by means of a straight-edge, wire, and plumb-bob, project that line to the bottom of the shaft, where it is recovered and continued a short distance, depending on the straightness of the gangway at the bottom, and subsequently used as a base-line in the survey of the mine. The method of operation can, perhaps, be more readily understood by a *résumé* of a survey just finished in what is known as Wottring's mine.

The general object of these surveys is to prevent the gangways in the mines from infringing on the neighboring properties, but in the case under observation it was desired to connect the workings already made with a shaft which was about thirty feet away from the face horizontally, and about fifteen feet above the gangway in a vertical direction. (See accompanying map.)

In order to avoid inconveniencing the operations in the mines, which are worked through the day, the underground survey is made at night, and, in general, about three hours' work above ground is necessitated in connecting the surface-lines of this survey with the surface-lines of other mines, etc. This is done on the afternoon before the survey of the mine, and the work is timed so that the last sight which establishes the line directly across the shaft, and which is the one upon which all those underground are directly dependent, is made at the time the miners quit work.

The reason for this is found in the fact that the ground in the immediate vicinity of the shaft, and for some little distance around, is floored with plank, which generally cannot be taken up without considerable trouble and some expense. The two tacks which represent the line are placed in the timbering of the shaft, so that the straight-edge can be laid against them, and as these planks are liable to be subjected to considerable jarring by the full or empty ore-bucket, and as a change of an eighth or even of a sixteenth of an


PLAN AND ELEVATION OF
WOTTRING'S ORE MINE.
GLENDON IRON CO.
JUNE 1878. E. C. JR.
0 10 20 30 40 50 ft.
SHADING SHOWS NEW WORK.

inch in the position of either of the tacks would vitiate, and probably invalidate the whole survey, the greatest care must be taken both in selecting a position as secure as possible, and subsequently in avoiding any jars, which might have the effect of disturbing either of the points.

In establishing this line on the surface which is to be continued underground, care must be taken to fix it in precisely the right direction. Many of the shafts are more or less twisted, that is, a projection of the timbering at the top would lie obliquely on those at the bottom, and this in itself would be very apt to deceive in the direction to be given to the line. The gangways at the bottom are not always square with the shaft, so that it is advisable to descend to the bottom and sight upwards (by eye) at the straight-edge laid across the mouth of the shaft, which is shifted by the assistant above to give it the proper direction, so that in the subsequent work the transit may be set as far from the shaft as possible. The line approximately established is afterwards definitely located with the transit, and the above-ground operations with the instrument are then concluded.

The various tools required having been forwarded to the mine during the day, the night's work is begun by the engineer descending with his transit to the bottom in the bucket. This has been found to be the only way of keeping the instrument from being severely shaken, as when lowered by itself it is rendered extremely liable to damage, no matter how carefully it may be packed.

The assistant above then lowers the plumb-bob by means of a reel and annealed iron wire of sufficient strength to hold the plumb-bob, which is made of cast iron and weighs about ten pounds. Difficulty was experienced in finding the right kind of string or wire for the suspension of the plumb-bob; at first seagrass fishing-line was tried, but with the weight of the plumb-bob attached it took almost an hour for one hundred and fifty feet of it to attain its equilibrium of tension and twist. The line used stretched more than ten per cent. of its length under the combined influence of the weight and the wetting it received from water dropping down the shaft.

Subsequently, fine copper wire, which had been unwrapped from the coil of a telegraph instrument, was tried, but proved to be too weak. Brass wire was afterwards tried, but with no better results than the copper wire, being objectionable on account of the amount of spring in it, which would cause it, when relieved of the weight, which happened several times by the breaking of the wire lower down in the

shaft, to uncoil itself from the reel to the extent of fifty to seventy-five feet, involving itself in knots and tangles of the most complicated description.

Finally, annealed iron wire was tried, and gave perfect satisfaction in every respect. The plumb-bobs are attached to the wire and lowered, which tends to keep the wire taut, but those below should be careful to stand well away from the shaft at this time, since should the wire break when the plumb-bob is near the top of the shaft, it will descend with sufficient momentum to penetrate a two-inch plank, and should it happen to strike the sides of the shaft or the other plumb-bob, it may glance and fly out into the gangway.

The two plumb-bobs are received into a large bucket filled with water at the bottom of the shaft, for the purpose of stopping their vibrations, which arise from several causes: First, the pendulum-like motion they receive while being lowered; second, the drippings of water from the sides of the shaft striking the wire; third, the influence of air currents up or down the shaft; and lastly, it is asserted that the difference in the earth's velocity at the surface and at the bottom of a shaft two hundred feet in depth, has the effect of causing a vibration in a pendulum of that length.

In some cases, the combined action of these vibratory forces is so great that the motion is merely lessened and is not entirely stopped by the water in the bucket; in cases of this kind mud, of which there is always a sufficiency at the bottom of a shaft, is thrown into the bucket until the mixture becomes of a consistency which will cause the vibrations to cease. This accomplished, the distance between the two wires is measured, both at the top and bottom; if they agree, it may be assumed that the wires are clear, but this should be proved by placing the lamp at the bottom successively on each side of the wires, and having the assistant above sight down the shaft along the wire; if he can see the lamp in every position, it is evident that the wire nowhere touches the side, but if it is hidden from view by one of the sides of the shaft, the wire must be shifted until the lamp comes in view.

An effort must now be made to place the wires at their extreme distance apart; this is done by shifting the wires, sighting with the lamps, and measuring alternately at the top and bottom.

The average distance in fifteen shafts surveyed in this manner by the writer, has been found to be between eighteen inches and six inches; that is, the plumb-bob wires were never further apart than eighteen inches, and never nearer together than six inches,

this distance being the base-line upon which the mine survey was founded.

In the survey at Wottring's mine the distance was twelve inches, and the wires were within one-half inch of touching the opposite sides of the shaft.

The wires having become stationary at the greatest distance they can assume from each other without touching the sides of the shaft, a temporary platform is built behind each of them, on which a miner's lamp is placed, trimmed to burn brightly, and the transit is set up as far as possible from the wires, and lined in with them. It seldom happens that it can be set up at a much greater distance than fifteen feet; and sometimes, from the crookedness of the gangway, or from a slight error in the direction originally given to the straight-edge, the distance will be only ten feet. In these cases an additional difficulty creeps in, consisting in the necessary change of focus of the instrument for the purpose of seeing the different wires.

As long as the two wires are seen through the instrument the line is not correct, but when only one wire is visible, the other being concealed behind it, the instrument is in an exact prolongation of the line on the surface above, and may now be reversed and the work continued as in an ordinary mine survey. The points on the bottom should be projected upwards into the timbers or lagging by means of a plumb-bob, and there marked by a "spud," which consists of a horseshoe nail with a hole crosswise through its head; this is driven into the timber and the number of the station marked in white paint for future convenience.

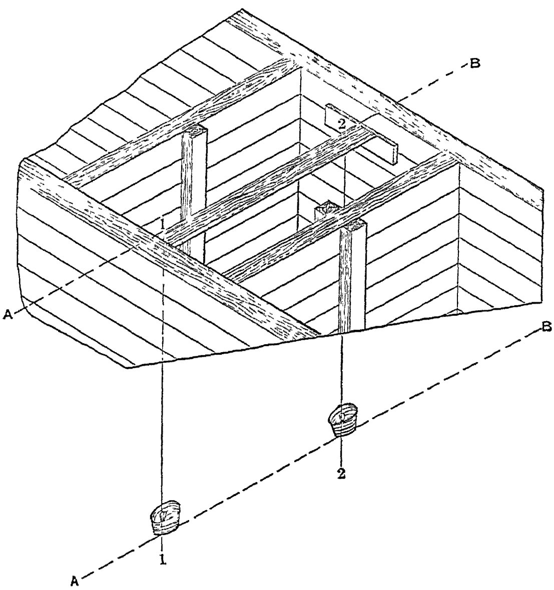
After the completion of the underground survey, the same line was run on the surface, and the direction and distance to the shaft ascertained. The course of the gangway was changed so as to point towards the shaft, and extended for the proper distance; a chute was then driven perpendicularly upwards, with the very satisfactory result of opening directly into the bottom of the shaft.

DISCUSSION.

MR. J. H. HARDEN: The method of connecting the angles of a survey on the surface with the angles of a survey underground, described by Mr. Clark, is one of the plans I and others have often used in the coal mines of Pennsylvania, but never with so short a a base-line as eighteen inches.

The plan pursued differs but little from that already described; it was as follows: A pair of cleats were nailed permanently within the cribbing on which to rest a straight-edge, and they, together with certain witness points (stations), distant from the shaft in the same line, were allowed to remain, both on the surface and underground, as points for future reference or reconstruction of the same base-line in future work.

The lines A B and A B on the sketch represent parallel lines on the surface and underground, their distance apart being equal to the depth of the shaft, their parallelism being adjusted by plumb-lines, 1 1 and 2 2, suspended from the straight-edge; their distance apart, 1 2, being equal to the length of the base-line, or width of the shaft.



By this method I have extended a survey down a shaft 400 feet deep and 3000 feet from the foot, in seventeen stations or angles, from a base-line 9 feet in length, giving the line for driving up the pitch 300 feet to connect with a slope already driven down 1100 feet. This, I think, could not be done very accurately from a base-line 18 inches long.

In the survey referred to by Mr. Clark, the two points to be connected are distant from each other about 125 feet; therefore an error of $\frac{1}{10}$th of an inch in the transfer as described, would make a difference of 8 inches in the surveys above and below ground.

EXPERIMENTS ON THE REMOVAL OF CARBON, SILICON, AND PHOSPHORUS FROM PIG IRON BY ALKALINE CARBONATES.

BY DR. THOMAS M. DROWN, LAFAYETTE COLLEGE, EASTON, PA.

In the course of some experiments on the analysis of pig iron, I heated, in a platinum crucible, some borings of a graphitic pig iron with sodium carbonate. When the crucible was at a full red heat and the carbonate was thoroughly fused, I noticed a lively ebullition of the mass, and an escape of gas, which burned on the surface. On investigation this gas proved to be carbonic oxide, and on examining the borings in the bottom of the crucible after an hour or two, they were found to be perfectly malleable, although unchanged in general form and appearance. Analysis showed that the carbon was completely removed.

I then repeated the experiment, to ascertain whether the silicon and phosphorus were affected, and found that these elements too were largely removed, and could be found in the fused mass as alkaline silicates and phosphates.

In order to ascertain whether the removal of the silicon and phosphorus was merely a surface action or whether the reaction really extended into the mass of the iron, I tried the following experiments:

A number of bars of pig iron were cast about one foot long and planed down accurately to one inch section. These were immersed in a large wrought-iron pot full of molten sodium carbonate made by fusing the commercial bicarbonate of soda. Through the kindness of Mr. Frank Firmstone, Superintendent of the Glendon Iron Works, Easton, I was enabled to keep this pot at a reasonably constant high temperature for a long time in one of the hot-blast ovens.

One of the bars was removed from the pot every twenty-four or forty-eight hours, according to the rapidity of the action, and after cooling was broken and the fracture examined. The change from pig iron to malleable iron was progressively inwards and always sharp and easily recognized, the rate of progression being in a decreasing ratio with the time. The temperature of the oven had evidently

some influence on the rate of progress, but a sufficient number of experiments were not tried to settle definitely what temperature was the most favorable. In some cases the iron was converted on the surface to a black oxide, and the oxidation of the iron once commenced progressed at the same rate as the oxidation of the carbon, while in other cases the carbon was removed to a depth of $\frac{3}{16}$th of an inch or more, while the surface of the bar preserved the original marks of the planer without a trace of oxidation. I do not know what were the conditions which determined the oxidation of the iron.

The following analyses of the successive layers of the bars subjected to this treatment, made under my direction by Mr. P. W. Shimer, then a student in the Mining-engineering Department of Lafayette College, show the nature and progress of the reaction between the carbon, silicon, and phosphorus of the pig iron and the alkaline carbonate.

Bar No. 1 was immersed in the bath for ten days, and the oxidation was noticed to have extended about $\frac{3}{16}$th of an inch. After removing a small layer of scale, two layers were planed off about $\frac{1}{16}$th of an inch thick each, and subjected to analysis, together with planings from the interior of the bar, and also of a portion of the same bar which had not been treated.

The following are the results in duplicate:

	Original bar.	First layer.	Second layer.	Interior.
Carbon,	3.576	0.115	0.269	3.587
	3.554	0.101	0.378	
Silicon,	1.384	0.824	1 059	1.383
	1.375	0.819	1.126	1.370
Phosphorus,	0 866	0 392	0.704	0.912
	0 872	0.494	0.640	0.910

The lack of close agreement in some of these duplicate analyses is without doubt due to the difficulty of getting average samples of the different layers.

Bar No. 2 was identical in composition with No. 1. It was immersed seven days and showed a slightly greater depth of conversion than No. 1, without the least oxidation of the iron. It was planed in three layers and the analyses resulted as follows:

	First layer.	Second layer.	Third layer.	Interior.
Carbon,	0.057	0.166	0.942	3.293
Silicon,	0.574	0.607	1.281	1.362
Phosphorus,	0.015	0.201	0.776	0.911

The first layer in bar No. 2 corresponds to the layer of scale or oxide in bar No. 1, hence the lower percentages of carbon, silicon, and phosphorus in the outside layer of No. 2.

A bar of white iron $\frac{5}{8}$ inch by $1\frac{1}{4}$ inch was immersed in the bath for ten days, and on removal it was found to be sufficiently malleable to be forged hot to a point. The analyses of this bar and of the original white iron are as follows:

	Original White Iron.	Outside layer.	Interior.
Carbon,	2.199	0.128	0 381
Silicon,	0.947	0.781	0.919
Phosphorus,	0.607	0.415	0.522

The reaction with the carbon of white iron is, as might be expected, much more energetic than with gray iron, and the conversion to malleable iron more quickly effected. No determinations of sulphur were made, but I think there can be no doubt but that it would share the same fate as the other elements.

Aside from the interesting character of this reaction of alkaline carbonates on the carbon, silicon, and phosphorus of pig iron, one which it seems to me was hardly to have been anticipated, it is further a matter of interest and surprise that the silicic and phosphoric acids in combination with the alkali should be actually removed through an appreciable mass of iron. The experiments were not continued long enough to determine to what depth the change could be carried.

The practical applications of this process will readily suggest themselves. The removal of the carbon, silicon, and phosphorus from thin plates of cast iron could be effected in a short time, and the process becomes at once one for the preparation of malleable castings; or, the converted product might, when it had a suitable composition, be melted for steel.

On the completion of my experiments, I was surprised to learn that Mr. A. K. Eaton, now of Brooklyn, N. Y., had discovered this action of alkaline carbonates on pig iron many years ago, and had patented the process in 1860. On consulting his patent specification I find that he fully recognized all the reactions which I have described. His claim was for a new process of making steel. To quote from his patent: "The bars converted into steel by this process may be worked directly under the hammer or in rolls, or may be melted, cast into ingots, and hammered."

Although I cannot claim more than having rediscovered a process which I think has been lost sight of, yet I think the analytical results given above, showing the nature of the process, will not be without novelty to some of the members of the Institute.

LAFAYETTE COLLEGE, EASTON, PA., September, 1873.

NOTE.—Since preparing this paper for publication, my attention has been called by Prof. G. J. Brush, of New Haven, to an article by Ch. Tissier, in the *Technologiste*, of July, 1857, page 357 (also in Dingler's *Polytechnisches Journal*, vol. cxlvi, page 118, and in abstract in the American Journal of Science (2), xxxi, p. 120), giving an account of experiments made by him on the action of sodium carbonate on cast iron. He recognized the removal of carbon and silicon, and inferred that the phosphorus and sulphur were likewise removed. Cast iron subjected to this treatment, he found became malleable.

T. M. D.

THE PRODUCTION OF CHARCOAL FOR IRON WORKS.

BY JOHN BIRKINBINE, PINE GROVE FURNACE, CUMBERLAND COUNTY, PA.

THE favor with which the members of the Institute received Mr. Fernow's paper upon the use of *charbon roux* in the manufacture of iron has encouraged me to present this paper, in the hope of having the discussion upon the manufacture and use of the oldest and undoubtedly the best fuel for producing pig iron continued.

Owing to the advances made in employing anthracite coal, bituminous coal, or coke, or mixtures of them, we are apt to forget how much of our present development is due to the pioneers in iron manufacture and the fuel which they employed in the production of pig iron. In Mr. Swank's last compilation of the iron and steel works of the United States, the furnace capacity of the country is given as 5,868,000 tons, of which 1,000,000 tons can be made in the 264 charcoal furnaces. In other words, over one-third of the furnaces now in existence use charcoal for fuel, and produce more than one-sixth of the iron made. If to this is added the 130,000 tons of blooms and billets turned out by the 122 forges and bloomaries, the importance of the charcoal industries will be appreciated.

In making the comparison of the operation of various charcoal furnaces, the writer has found difficulty in arriving at proper conclusions, owing to the variety of bushels in use. They have been found to be rated in capacity from about 2480 to 2748 cubic inches, and in weight from 18 to 22.5 pounds; two furnaces in one neighborhood have been noted where the standards were respectively the two extremes of weight mentioned, although the timber used was of approximately the same varieties and proportions.

It has therefore been necessary to reduce the fuel consumed per ton of iron to cords of wood, as a cord seems to represent a fixed quantity in all districts (except so far as the ingenuity displayed in piling by some woodchoppers affects its interstitial spaces). The records of fifty charcoal furnaces show an average consumption of 138 bushels per ton of pig iron, and a yield of 35 bushels per cord of wood would represent the consumption of four cords of wood per ton of pig iron.

An allowance of four cords of wood per ton of pig iron produced is probably too low for a general estimate, but taken upon that basis the charcoal blast furnaces of the United States have a capacity for consuming annually 4,000,000 cords, and fully 400,000 more are employed in the forges and bloomaries.

Making allowance for fires, the average growth of timber in the vicinity of the iron works will not exceed one cord per acre per annum. Therefore, to supply these furnaces, forges, and bloomaries will necessitate the denudation each year of 147,000 acres, or 230 square miles, of thirty-year timber. And yet this large quantity represents but a fraction of the woodland which yearly falls before the axe.

It is not the purpose of this paper to discuss the climatic effects of this extensive clearing of lands, nor to consider its influence upon the water-powers of the country, for this more properly belongs to the sadly neglected department of forestry, which might well be studied by all metallurgists using charcoal for fuel.

Although we are credited with being in the steel age, the production of charcoal iron is not a manufacture of the past, nor is it likely to be abandoned in the very near future, and a discussion which will lead to more economical production of this fuel will be of value to many metallurgical industries.

Probably more than eighty per cent. of all the charcoal consumed in this country is produced in heaps or meilers, in the same manner that our grandfathers manufactured it. The mere fact of following in a path beaten by our ancestors is no condemnation of a process;

but it is well known that in this case the means employed give but a small percentage of the possible product. Kilns have been employed at a number of furnaces, but although the yield has been increased, the additional expense of hauling wood in place of charcoal has been offset against their advantageous employment, and except where water communication can be had their employment is the exception rather than the rule. Retorts have been used to a limited extent, but have been subject to similar objections.

A general impression exists among furnacemen and forgemen that charcoal made in any other way than in meilers is of inferior quality. There seems to be no good reason for this, for the carbonization in retorts and in moderate-sized kilns should be under even better control than in the meilers.

One cause of inferiority in charcoal made in retorts may arise from the possibility of carbonizing rapidly, for the valuable researches of Karsten and Violette, although controverted by Dromart, leave the balance of proof in favor of slow charring, both for quantity and quality of product.

It is probable, too, that Violette's comparative experiments upon charring wood under variable pressures have not been favorable to the employment of closed vessels; but the results are not fair indices, for the experiments were made in hermetically sealed tubes, the pressure upon which was at times sufficient to cause their rupture, while in practice retorts would work under but slight pressure.

The assertion made by Percy that "charring in retorts heated externally is not specially within the province of the metallurgist," which precedes his elaborate description of all other methods of carbonizing wood, may, on account of the standard of authority, have discouraged experiments in the use of closed vessels.

By properly regulating the heat applied to a retort the carbonization of the wood it contains may be either rapid or slow, as is desired to give the best results as to product. And since so much of the wood in a meiler is consumed in carbonizing the rest, that the ordinary yield of charcoal is less than twenty per cent. of the weight of the wood in the heap, and since this percentage of yield can be doubled in retorts, and the otherwise waste by-products utilized, it would seem that the employment of closed vessels would be the process of the future.

The varying success heretofore attending the use of retorts at iron works may influence their employment in different localities according to the results attained. That the quality of charcoal made and

the rate of carbonization will be under more complete control when a small amount of wood is treated in a closed vessel than when a larger quantity is prepared in an open pile, may be fairly admitted.

One objection to the use of retorts, and a very strong one, is the expense of constructing and maintaining the necessary plant, but it would seem that the products of distillation would more than compensate for the interest on the investment and deterioration of the plant. The number of acetates used in commerce and the large quantities of them employed in our varied industries, would encourage faith in the continuance of a good market for any material manufactured from the distillation of wood, and the increased yield from the same acreage would augment the value of the timber-lands available for charcoal production.

The crude pyroligneous acid is one of the best preservative agents, and is largely employed in preserving meats, vegetables, timber, etc. The wood spirit is utilized in fixing colors, dissolving varnish, etc. The various acetates are largely employed in calico printing, dyeing, and the manufacture of dyes and colors, and they are, also, to a greater or less degree, disinfectants.

The local demand for the different acetates, or the production of certain metals in the vicinity of the charcoal industry, may influence the character of the product. Thus it would appear that the Missouri charcoal furnaces might be able to cheaply manufacture sugar of lead, and that in the Lake Superior copper region verdigris or Paris green could be economically produced.

Being desirous of obtaining information concerning the possible yield of by-products, inquiries have been made of various parties conversant with the subject, with a view of determining the yield and commercial value of the various acetates.

The following data were furnished by M. Antoine Mathieu, a French chemist and expert in wood distillation, and are given on his authority. One cord of oak wood will yield in retorts 70 bushels of charcoal of 2561 cubic inches, and 225 gallons of pyroligneous acid, also $\frac{1}{2}$ to $\frac{3}{4}$ gallon of wood spirit and 25 to 30 gallons of tar.

The present prices in Philadelphia are about as follows: Pyroligneous acid, $2\frac{3}{4}$ cents per gallon; wood spirit, 75 cents per gallon; tar, 8 cents per gallon.

The pyroligneous acid can by a simple process be transformed into acetate of iron, by using about 40 pounds of iron filings, chippings, or detinned scrap; the acid from one cord of wood will yield about 220 gallons of acetate of iron, the present market value of

which is 11 cents; or by treating 40 pounds of quicklime with the pyroligneous acid from one cord of wood, 200 pounds of acetate of lime can be obtained, the commercial value of which at present is 4 cents per pound; or the resulting pyroligneous acid from one cord of wood may be made into 350 pounds of acetic acid worth at present 5 cents per pound. By submitting 175 pounds of lead made into litharge to the action of the pyroligneous acid from one cord of wood after it has passed through one distillation, 300 pounds of brown sugar of lead can be obtained, the quotations of which are at present 7 and 7½ cents per pound. If a portion of this crude acetate of lead be refined, a product of white sugar of lead valued at 19 cents per pound is obtained; or by similar action upon 80 pounds of copper 200 pounds of acetate of copper can be produced, worth at this time 27 cents per pound. Another valuable commercial product is the acetate of alumina, which can be produced from brown sugar of lead or acetate of lime and alum, or with acetic acid and clay. Wood vinegar can also be produced.

The above quantities and prices will permit of a considerable reduction, and yet show that a large amount may be expended in apparatus for producing some of the above products.

With the exception of Marcus Bull's monograph, read before the American Philosophical Society in 1829, most of the data we possess in reference to the yield of different woods, their calorific powers, etc., are gleaned from foreign publications, mainly French, German, and Swedish, and the writer is not aware of any comparative experiments in the manufacture of charcoal in round meilers, rectangular piles, kilns, retorts, etc., made in this country.

Out of 100 parts of wood placed in a meiler, say 20 parts are made into charcoal, 50 parts are burned to carbonize the 20 parts, or are carried away as acetic vapors; and 30 per cent. is hygroscopic water and uncondensable gases.

With such a waste, amounting to 50 per cent. of the wood, surely the subject of increasing the yield of charcoal and collecting the products of distillation is worthy of attention.

Is not the item of hauling the wood estimated unnecessarily high? The problem is generally stated thus: if wood is coaled in meilers in the woods the charcoal only is hauled, but if the wood is hauled to the kilns or retorts at the furnace five times the weight must be handled. This does not make any allowance for increase of yield in the latter methods. If retorts double the yield the item of hauling would be as 2½ to 1, and not as 5 to 1.

But why should not the retorts be used in the woods? There are numerous steam saw-mills throughout the country which are being constantly moved from place to place, and it is found to be economical work if but a few acres are to be cut. With the conveniences of special shapes in wrought iron, a semi-portable arrangement of retorts could be made, and it is probable that they would need to be moved but once a year. There are many tracts of woodland where such an apparatus could be set near to a stream, and the wood from 100 acres brought to it by sledges, with a maximum haul of say 1500 feet, and the charcoal delivered to the furnace or forge in wagons or shipped in bags on cars.

It is hardly fair to assume that charcoal cannot be transported by rail economically when less of it is required than of coke to make a ton of pig iron, while the latter is carried hundreds of miles.

The use of retorts for carbonizing wood is not new to us, but the records of failures are rather in excess of the successes; yet from these failures will surely be developed a practical economical method of carbonizing wood, and utilizing the now waste products.

Although full information in reference to the results attained is not at hand, I am led to the belief that failures may be attributed to the following causes:

1st. Deterioration of product, either by too rapid charring, or by unequal charring, owing to the arrangement of applying the heat; or to the fact of operating the retorts so as to give the best results in by-products to the detriment of the charcoal.

2d. Operating an apparatus, the success of which depends upon chemical combinations, without a knowledge of chemistry.

3d. Constructing the apparatus rather to save money in the plant than to reduce the expense of manufacture.

It surely does not appear too much to expect that the problem of making good charcoal upon an economical basis for metallurgical processes, and utilizing the valuable by-products may be practically demonstrated.

The immense waste by the present mode of carbonizing wood generally employed seems the more unnecessary, when we remember that wood is carried at heavy expense to our cities, to be subjected to destructive distillation in chemical works or textile fabric manufactories for the purpose of utilizing the acetic vapors.

These notes have been made with reference to the production of black charcoal as generally used, without considering the economy of using semi-charred or kiln-dried wood.

DISCUSSION.

Dr. Egleston: I think that in the burning of charcoal on a large scale for metallurgical uses, the manufacture of pyroligneous acids and the acetates is a step in the wrong direction. Experiment, in a great many of the best works, has proved that either the wood must be be distilled for the manufacture of these products, and the charcoal be a by-product, or else if charcoal is manufactured it costs more to save the acetates, etc., than they are worth in ordinary times. It is practicable to make good charcoal and at the same time make the acetates, but in that case it has generally been found that the charcoal costs too much; and as the manufacture of good charcoal and acetates, at the same time is not generally advisable although quite possible, the collection of the products of distillation in the manufacture of charcoal on a large scale for metallurgical uses has generally been abandoned. This may be owing partly to the fact, that the chemicals are generally made in some kind of furnace, and that until within comparatively a few years, kilns and furnaces of all kinds for the manufacture of charcoal have been looked upon with very great suspicion. It is not many years since there was an almost universally received opinion amongst iron manufacturers, that charcoal made in kilns did not give so great a yield, and that the product had a low calorific power. This was an opinion brought from Europe as the result of long experience there, and seemed to be well founded; as a furnace for charcoal manufacture must be in some permanent position, while the wood necessarily never can be, but must be transported. As all wood contains a large amount of water, the best at least, 20 per cent., one-fifth of the material had to be transported to no purpose. Little by little, however, this opinion has been abandoned, and the larger part of the charcoal manufacture in the United States is now made in kilns of a great variety of construction, and considerable variety in yield. I hope in the course of a few months to present to the Institute a paper which I have been for some time preparing, showing the different styles, yield, and cost of the manufacture of charcoal in different States of the Union. It was at first thought that the larger the kiln the better. We are now, however, gradually giving up this opinion, and find that the best yield comes from kilns which contain only about seventy-five cords of wood.

When the products of distillation are to be collected, retorts are generally used for carbonizing the wood, but kilns and piles have

been used in Europe; and it is their experience imported into this country which has resulted in the opinion which I have cited.

There has been in times past great profit in making acetic acid, acetate of lime, and other products of that kind, but the profits of these works and their number have been steadily diminishing for a number of years past. The process of collecting, separating, and condensing the products of distillation is so complicated, and requires on the part of the workmen such a different training and judgment from that required to make charcoal only, that there must generally be a sacrifice of one or the other of the products to insure success, for the conditions of producing the greatest amount of acetates, are not those for producing either the best or the greatest amount of charcoal.

The almost uniform results of the chemico-charcoal factories has been that either one or the other of the products has been a by-product, not that the charcoal was not good, but it was sold at a low price because it was not the principal object of the manufacture. I have undersood that there are blast furnaces using the charcoal from such distillation, but the chief reason of its use was that it was cheap, because, in order to get the different chemical products in greatest amount, the conditions are not favorable to the manufacture of large quantities of good metallurgical fuel. If, however, there is a demand for the charcoal, then the chemical products may become by-products, and can be manufactured of a poorer quality or in less quantity. If, again, the chemical products are to be the principal, then the charcoal may become incidental, and in certain places where factories of this kind have been established with the object of producing an excellent acetic acid, they have found it more profitable not to carry the distillation so far as to make charcoal, but to manufacture a torrefied material which is sold for kindling wood.

In order to get the greatest yield of chemical products it is desirable to cut the wood up in such small pieces that it is unfit for metallurgical uses, and to prevent the process being carried so far as to make charcoal, it is conducted with such care, that the men are compelled to deluge the retorts with water when the charcoal product begins to form.

These manufactories are usually, though not always, situated near large cities. The constant danger from burning makes them a nuisance, and has, in several instances that I know of, forced the manufacturers to remove to districts and centres less populated.

I think that experience has shown that the two manufactures cannot be carried on simultaneously without sacrificing one to the other, and hence very few of the charcoal-burners of this country, who make charcoal for metallurgical uses, collect the products of distillation. All the chemical manufactories make a product which is either charcoal or is allied to it, which is sold as a by-product; but the higher the grade of chemicals they manufacture, the less valuable the charcoal; and the more valuable the charcoal, the more impure the chemical products. I have heard of several works, where the attempt to pay equal attention to both products, in order to produce a high grade of both, has resulted disastrously. I am informed that the proprietors of the establishment which makes the purest acetic acid finds it for their interest not only not to produce charcoal at all, but also to take special precautions against it. A simple inspection of the complicated apparatus necessary to condense and separate the volatile products, would, I think, convince almost any capitalist who wished to manufacture charcoal, that except in unusual cases, his interest was to look for the best quality of charcoal; and the chemist that his was to get pure chemicals, and in both cases to let the intermediate products be minor considerations.

MR. FERNOW: The discussion of the theories of charcoal-burning and descriptions of processes based thereon (and there exist by-the-bye, about half a dozen more, and these essentially different from those mentioned here this evening), was carried on in the technical journals of Germany and France, during a period from 1836 to 1860. The subject was then dropped, presumably on account of the high price of wood in those countries which made the charcoal iron manufacture recede to insignificance, and with it the interest in this question. What was needed to settle the relative merits of the different methods was simply practical trial by competent men; of opinions there had been a surfeit.

About sixteen years ago the Swedish Government offered a prize for a good handbook for charcoal-burners. The first four competitors failed entirely in their attempts, nor did a second offer, for which there were seven competitors, meet with much better success. The prize was, however, divided between two authors, and their manuscripts were given to a third to compile the book. I have seen this book, and it is not much better than I could have written myself. The movable retorts mentioned by Mr. Birkinbine were used some twenty years ago by Count Von Reichenbach, at Hradau in Bohemia, with success. The enterprise failed, however, because the cheap ace-

tates manufactured did not pay the transportation to the distant railroad in those times.

MR. BIRKINBINE: The "other processes" referred to by Mr. Fernow are not essentially different from those mentioned, for they all come under the three classes of meilers, kilns, or retorts. Chabeaussiere's process is merely a kiln excavated in, instead of being built upon the ground; and the same may be said of the Chinese sunken pits described by Percy. The shroud or abri is but a movable covering for a meiler. But there are numerous modifications of the various classes in use in different localities.

I would refresh Mr. Fernow's memory by recalling the fact, that the charcoal-burner's handbook as prepared by Svedelius, is devoted entirely to the consideration of manufacturing charcoal in meilers, and only in appendices is mention made of kilns or retorts. Of the latter no data at all are given.

Prof. Eglcston's statement, that the larger part of the charcoal made in the United States is produced in kilns, does not agree with investigations which I have made, and I believe considerably the greater portion is obtained from the more wasteful meiler charring.

Admitting the force of his remarks in regard to the simultaneous product of charcoal and acetates, it seems to be against our progressive natures, to condemn a process because it has been imperfectly carried out. To produce charcoal for metallurgical purposes, the acetates should certainly be made "by-products," as was mentioned in the paper. But to collect these by-products, and utilize them economically, demands the attention of an expert in wood distillation.

Had the same amount of study been given to charcoal production in proportion to the quantity consumed, as has been devoted to the getting of mineral fuel, I am convinced that some practical method would have before this been found, with all the advantages of cheap charcoal, good yield, and collection of acetic vapors.

THE BUTLER MINE FIRE CUT-OFF.

BY HENRY S. DRINKER, E. M., PHILADELPHIA.

THE Butler Mine property is situated in the vicinity of Pittston, in the Wyoming coal-field of Pennsylvania. The coal has been worked out from the fourteen-foot or Baltimore vein for a number of years on part of the tract, the old chambers remaining open as when originally abandoned. This vein outcrops on the Butler property, and is nowhere more than sixty feet below the surface of the ground on the line of the cut-off to be described. Above the fourteen-foot vein and about, say midway, between it and the surface of the ground, there is another small vein or rider of coal, about twelve inches in thickness. The rock between the two veins is a carbonaceous slate, about twenty-five feet in thickness. Above the smaller vein is a bed of sandstone, varying in thickness, but generally not less than from eight to ten feet. This sandstone is also carbonaceous, but the proportion of carbon is much less than in the slate below. Both rocks have been analyzed, and before the next Institute meeting, the writer hopes to be able to furnish the exact proportions. The sandstone is overlaid with *débris* up to the surface of the ground.

Some time during the spring of 1876, it was found that the coal left in the worked-out mine had been set on fire, and that the fire was gradually spreading. Inquiry developed the fact that the fire had originally been started by a woman, who had taken up her abode in an old tunnel leading into the workings. Owing to some delay in the early attempts made to extinguish the fire, it finally gained such headway that no ordinary means were found to be effectual. Finally, Mr. C. F. Conrad, civil and mining engineer, of Pittston, Penna., was called in as consulting engineer. He advised the making of a thorough cut through such portion of the old workings as had not been reached by the fire. It was found by survey that this cut could be limited to a length of about one thousand two hundred feet, as the vein faulted on both sides, and that the greatest depth of the cut would be about sixty feet. Subsequently it was decided to tunnel part of the way, thus saving the deepest portion of the cut.

In 1856–7, there occurred a fire in the same mine. Two stone-walls in V-shape running from the apex of the V towards the outcrop, were built to cut the fire off, and these walls were carried as far as the vein had at that time been worked. After the walls were finished

the old workings were filled in on both sides with clay and sand, making the spaces air-tight; this helped to stay the fire; it, however, finally broke through to the surface of the ground, and only went out when it reached the solid coal.

The present cut passes through the point of junction of the old V walls, and is, therefore, located directly through the *débris* and ash of the old fire. This location for the cut-off was decided on by Mr. Conrad, after a careful survey, for two reasons. 1st. In the hope of finding all combustible matter, coal, gob, carbonaceous slate, etc., burnt to ash by the old fire. In this case much excavation would be saved, as the ashes of an extinct fire might fairly be assumed to be an impassable barrier to a new one, and 2d. In the hope that if the material were not found to be fully calcined, still that its burnt condition would render it more workable, and thus more easily removed.

After removing the surface material the walls above described were found, and then about 25 feet beyond them a solid pillar of coal in place. Where this opening was thus first made to the old fire, the covering over the vein was from 30 to 35 feet thick—about 3 to 5 feet of surface soil and earth, and the remainder yellow slaty sandstone and coal slate, the slate and the sandstone being parted by the rider or small coal vein above noted. It was at this point that a curious fact was developed, and it is in order to bring it to the attention of the Institute that the writer has prepared the present communication. On investigation, it was proved that the old fire had not penetrated the solid coal *in place*. Where it met a pillar in the workings, the coal was simply calcined on its surface; but in all cases *the overlying slate rock was calcined throughout*. Nay more, for the 14-foot vein on the Butler property is nearly equally divided by an 8-inch parting of slate, and it was found that this slate rock (with sound bright coal on both sides), had been thoroughly calcined. Above the 14-foot vein the rock was also found to be calcined up to and above the 12-inch rider, or small vein, while this small vein of solid coal was also found to be unburned, and perfectly sound and bright, as in the case of the larger vein.

Finally, it was found on excavating further, that when the old fire had reached the face of the workings, its progress had been arrested.

From this experience it would appear to be established that coal *in situ*, *cannot* be burned *en masse;* but that the walls of carbonaceous slaty rock inclosing solid coal, *can* be burned or calcined *in*

situ. On what theory this fact can be explained, may be an interesting matter for discussion to some members of the Institute, more directly connected, than the writer can claim to be, with problems of this nature. At the February meeting (1879), the writer expects to be able to present samples of the coal and rock referred to, both in its calcined and in its natural state, and by that time he hopes also to have the analyses above referred to, showing the proportion of carbon in the slate and sandstone. It should also be stated here, that even the sandstone was found to be burned, though not so thoroughly as the slate.

At present the question to the company, of course, is, whether the new fire, now raging, will be stopped by the cut or not. The cut was made about 30 feet wide, and is carried from one end of the coal to the other, and down to the footwall of the vein. About 200 feet of the distance has been tunnelled. At this point the vein has been removed, and heavy walls built on either side of the tunnel. Also the adjacent old workings towards the fire have been filled with clay and incombustible *débris*, and it is believed by the engineer in charge that this, with the cut, will afford an effectual stay to the fire, which is expected to reach the line of the cut some time in January or February, 1879. When the present fire becomes extinct, it will be an interesting matter to note, whether in the old workings in which it has been raging, the pillar coal has remained unburned as in the old fire. That the superimposed slate and sandstone is being calcined is evident, for the craters formed over the surface, show on all sides at night, glowing masses of incandescent rock, extending often up to the surface.

The writer hopes to be able to supplement the present paper by another at some subsequent meeting, giving a full account of the construction of the cut-off and of its result. This, of course, cannot be done until the fire has progressed farther, and actually reached the cut.

IMPROVED PIPE AND TUYERE.

BY JOHN M. HARTMAN, PHILADELPHIA.

The high temperature of the blast of modern furnaces renders it desirable that the pipe conveying the blast into the furnace crucible shall transmit and radiate as little heat as possible.

To accomplish this, the pipe shown in the accompanying sketch was devised and patented. It consists of a slide-valve, leg-pipe, and belly-pipe, which are held together and to the tuyere by a system of levers and links.

The slide-valve is superior to the old throttle-valve. The leg-pipe is formed of two thicknesses of wrought-iron pipe, with a space of half an inch filled in with clay and brick. The belly-pipe is made in the same way as the leg-pipe, and terminates in a globe face to suit the tuyere. The quarter turn is made extra thick, of cast iron, and the cap on the end is lined with clay also. The catches, A, B, C, are merely for convenience in putting the pipe together.

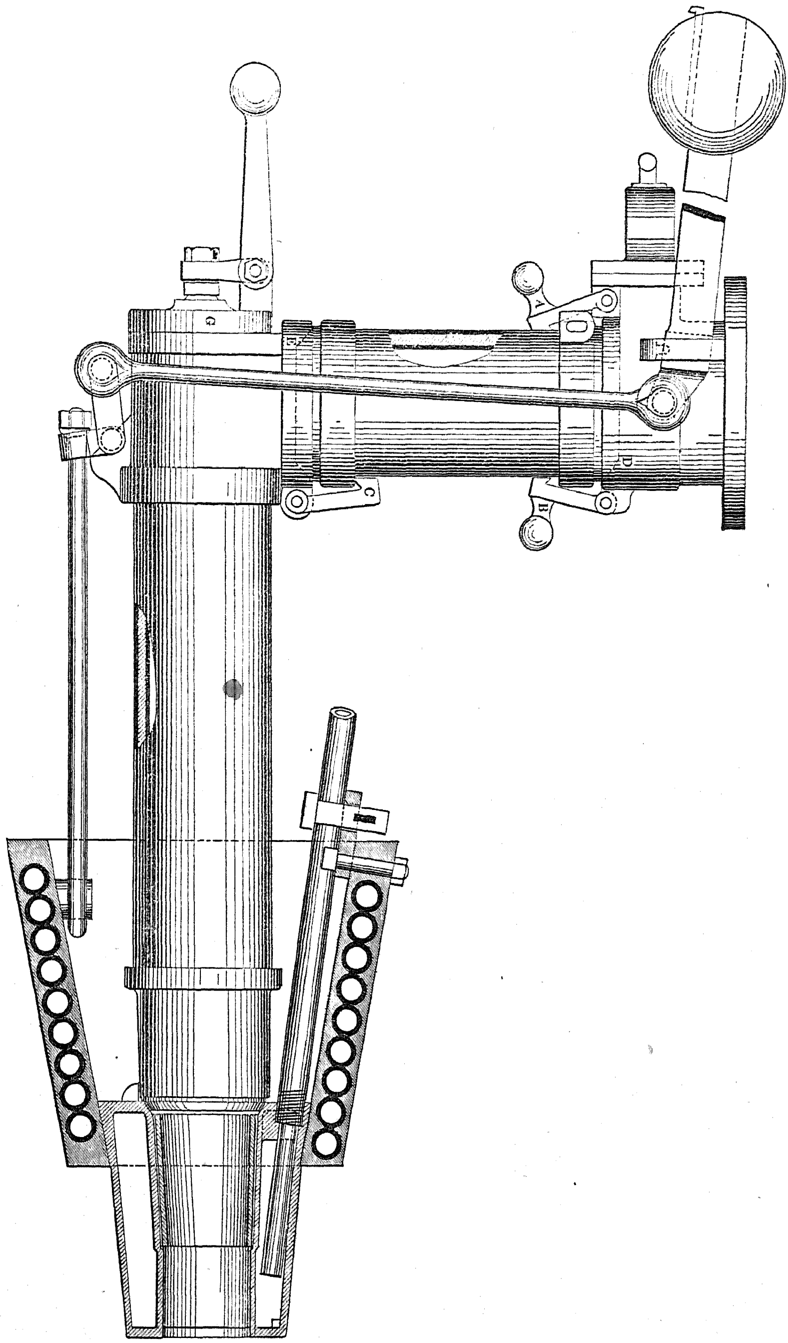
The joints D and E are globe faces, and allow the pipe to adjust itself to any angle. The eye-piece is made of violet glass, which cuts off the glare from the bright coals in the furnace and allows a close examination of the interior, so that any water dropping or vapor can be detected. The glass does not become coated with dirt, like mica. Since good running of furnaces is shown by a bright tuyere, it is necessary to have a good eye-piece to examine them.

To detach the pipe, a man lifts the weights, the side rods are then slipped off the bell crank under the quarter turn, the link under the belly-pipe is unhooked, and the pipe carried away. The same process is reversed to put the pipe together.

The nozzle is changed by taking off the cap, G, drawing the nozzle through the belly-pipe, and inserting another. In this way the change can be made quickly, without taking down the pipe.

On the old tuyere pipe bolts with brass nuts were used, which burned up and gave trouble. In other cases key-bolts were used, making it difficult to adjust the pipe.

Bolts make too rigid a joint, and there is no provision for movement from contraction or expansion. As the temperature of the blast increases there is more expansion, and where rigid joints are



used they must be eased up, or leakage will start at some other point. What is required is sufficient force to hold the joints together and have them flexible and yielding at the same time. The rods and levers give this flexibility to the pipe, dispense with the bolts, and allow for expansion.

After two years' trial this system of levers and links for holding the pipe together has been found to do well. It especially recommends itself for quickness in taking down and putting up the pipe.

The loss from transmission and radiation of tuyere-pipe has received but little attention. Take the case of the old tuyere-pipe, which averaged 11 square feet of surface, used in connection with a 4-inch nozzle. This would deliver 1700 feet of air per minute, say at 1200°, with a total loss of heat of 8 per cent. This loss, in the case of the new pipe, is 3½ per cent., and the difference between the two represents a loss of 105 tons of coal per year for a 17-foot furnace.

The bronze tuyere here shown is the T. F. Witherbee patent. It has been adopted by twelve iron works, and is rapidly gaining favor.

Bronze tuyeres stand better than iron-coil tuyeres, especially in connection with fire-brick stoves; they keep a clean nose, allowing nothing to adhere firmly, protect the nozzle, deliver the blast solid into the crucible, avoiding the dispersion of the coil tuyeres, and stand drilling better. In event of a tuyere giving out, it is only a small crack that opens, which does not allow a large quantity of water to get into the furnace. To remove a tuyere it is only necessary to put a hook in it, and pull it out; there is no digging required, as nothing burns fast to it.

The plan adopted by some of partly digging out the coil tuyere while the blast is on, to save time of stoppage, is dangerous. At the Keystone Furnace, recently, one of our best founders and a helper were badly burned, the pressure in the furnace blowing out the tuyere and throwing hot coals and cinder over them.

The Witherbee tuyere is a short double-jacket bronze tuyere, turned to fit the end of a water-breast. On the end of the tuyere is turned a globe face, to suit the end of the belly-pipe, which latter is held to its place by the link under the pipe. By this arrangement the old clay packing is dispensed with. The globe face, being water-cooled, retains its shape even when cinders back into the pipe. Lengthwise of the tuyere, at the top, between the jackets, is a division which forces the water to circulate around the nose of the tuyere, where a small opening is cut through the division to allow

any air to escape directly into the discharge. A similar escape is placed at the butt end of the tuyere.

The cost of a 4-inch Witherbee tuyere is $28, which includes the royalty, but where the right to use the tuyere is purchased the price is $24; when they are worn out they are worth $12. The cost of a coil tuyere is $12 to $13, and they are of no value when burned out; they would therefore be no cheaper than the others, even if as durable, but the bronze tuyeres outlast the coil tuyeres 3 to 1 on competitive trials, which makes them three times cheaper, without taking into account the lost time.

The time required to change a bronze tuyere is one-third that of the coil tuyere. If we allow one hour to change a coil tuyere, and say one tuyere a week is lost, then fifty-two hours a year are lost, while with bronze tuyeres fourteen hours only would be lost. If we assume that the bronze tuyeres outlast the coil only 2 to 1, then the difference in time lost in the use of the two styles would represent an additional yearly production of 112 tons of iron.

Though great and revolutionary improvements in blast-furnace practice are not made every day, yet it is still possible to attain a greater economy in the production of iron by attention to the minor details of construction and working.

Discussion.

Dr. R. W. Raymond: I can confirm from experience with bronze tuyeres at the Durham Iron Works, the statement of Mr. Hartman as to their keeping clean at the nozzle. The tuyeres were, it is true, removed from the Durham furnace after some months, and the common iron-coil tuyeres were substituted. The reasons were mainly economical. At that time the bronze tuyeres cost $90 apiece, and the metal was worth but $45, leaving $45 as the loss every time a tuyere was burnt or otherwise destroyed. The coil tuyeres, on the other hand, cost $10 or $12, and reckoning the burnt tuyeres as worthless, this sum represented the loss. The question in practice became, therefore, simply this: Was the number of iron tuyeres lost from all causes in a given time four times as great as the number of bronze tuyeres lost from all causes under similar circumstances? This question our experience answered, for our own case, in the negative; and we threw out the bronze tuyeres in consequence, not because they were not in some respects better, but because their

superiority was too dearly paid for. The design which Mr. Hartman now exhibits is better in construction and much cheaper in first cost than the bronze tuyeres which we used, and I expect, on putting the Durham furnace again in blast, to make another trial, under these more favorable auspices, of the system he advocates.

MR. HARTMAN: The large bronze tuyeres at the Durham Iron Works compared well with the coil tuyeres for durability, but being larger and longer than was necessary, they cost too much. The large surface exposed to the fire made them more liable to burn than the present tuyere, which has less than one-half the surface. Further, the present tuyere is composed of nearly pure copper, while the Durham tuyeres were harder and more liable to crack. We have worked hard to overcome the difficulties enumerated, and think we have now got a good tuyere.

THE WHEELER PROCESS FOR WELDING IRON AND STEEL WITHOUT THE USE OF FLUXES.

BY D. TORREY, PHILADELPHIA.

CONSIDERING the two great interests of to-day, in iron upon the one hand and steel upon the other, and recognizing with measurable distinctness the peculiar fitness of each for special services to which they are applied, the proposition to unite the two metals in combination, to bring into simultaneous use the peculiar advantages which each offers, has great attractions. The limited success which has been attained by trying to bring about such combinations of the two metals through the use of fluxes in welding, has led to the general adoption of the opinion that an effective union of the two metals is impracticable, or to the view that if practicable it would have but limited commercial value.

Such opinions are, I think, premature and erroneous, and having gained some knowledge of the character and value of the "Wheeler process" for welding iron and steel without the use of fluxes, I submit the following observations and opinions of the matter for your information, with the hope of eliciting such a discussion of the subject as it may require.

The principle of the invention is to bring the two metals simultaneously to a welding heat in one pile, and then to manipulate it.

The one important provision, the key of the situation so to speak, is to effect, while in the furnace and immediately afterwards, a practical exclusion of oxygen from the surfaces of steel intended to be welded. This is done by boxing or inclosing the steel with plates of iron, which plates, made to lie in overlapping contact at the corners of the pile and with its end plates, are found to effect a satisfactory exclusion of an oxidizing atmosphere. Such a pile, heated gradually so as to permit the suitable penetration of the heat through its mass, bringing the steel into a condition of semi-fusion, is given at last a wash-heat to bring the iron casing to a proper welding temperature, when the whole pile, that is the box with its contents, is passed through the rolls. The only precaution to be taken at this stage of the process, is to avoid making too great a reduction at once, lest the semi-molten steel should from too great pressure burst the iron casing and be wasted. After one or more passes through the rolls the welding of the iron to the inclosed steel will be attained effectually, and the steel, when of more than one piece, will be welded into a homogeneous mass.

This process is simply a method of welding, a process which offers for our consideration nothing in any way anomalous, and is one, the laws of which are found upon examination to harmonize with those of other metallurgical processes commonly practiced. We have for a long time transformed quantities of steel scrap into homogeneous masses by melting it in refractory crucibles, which in use are covered to exclude the atmosphere; while in the Wheeler process we use a malleable crucible which we feed to the rolls of the mill, and the material of which is united to its contents and becomes part of the product. We do not necessarily melt the contents of our crucible as completely in the new practice as in the old, but we do so in a sufficient degree to secure homogeneity of product.

In making our boxes we can use iron of variable thickness, so as to give that amount which will be serviceable in the finished article; the iron must, however, be thick enough to withstand the tendency of the semifluid steel to burst free from its confinement when the pile is first passed to the rolls, and also that the different parts of the case may be properly fastened to each other and to the pile. The viscid condition of iron at a welding temperature causes it, even when quite thin, to yield without breaking to the pressure of the steel to a degree which would scarcely be anticipated. The iron covering accommodates itself to every shape into which the mass it incloses may be worked, notwithstanding the unequal ductility of the metals, so that the resulting product, when manipulating steel by

this process, will be articles of iron-clad steel, known by the concise name of "combination."

The covering, or iron casing, serves a number of purposes in connection with the furnace treatment of the steel: it protects the steel from oxidation, or burning; the steel probably maintains, approximately, the percentage of carbon, which it had before treatment; it permits the steel to receive a high temperature, and its consequent greater softness allows it to yield to more rapid and effective reduction and shaping than if given only a steel heat.

A variety of methods of encasing the steel have been found to work satisfactorily: pouring melted steel into prepared iron boxes, casting ingots of steel and afterwards encasing them with plates of iron, and finally boxing, according to convenience or utility, special shapes of steel or any form of steel scrap.

The process requires that the external part of the pile shall be heated to the welding temperature of iron, which is about 2700° Fahrenheit, and as steel melts at 2552° Fahrenheit, or less, it necessarily happens that the internal part—the steel—will be in a molten or semi-molten condition when the pile is first passed to the rolls. The pressure of the rolls brings into perfect contact the opposite surfaces of porous cavities and seams in the body of the steel, as well as adjoining surfaces of separate pieces of steel, and of the steel and iron, which contact in the viscid or fluid condition of the metals becomes an unquestionable weld. For these reasons I say the process effectually effaces all traces of porosity and seams in the steel.

The usual conception of the nature of the union between metals welded together, may have to be modified in applying it to the weld made by this process, for the union between the iron and steel appears to be as perfect as is possible between dissimilar substances, the two metals being melted into union. The weld between two pieces of ingot steel or ingot iron, protected from oxidation, is, I believe, like that of platinum, a non-oxidizing metal,—one of perfect metallic contact. I would compare the union to the regelation of fractured ice, or like cases in which the cohesion is no less at the place of union than in sections where the mass has never been severed. And further, the partial decarburization of the steel near the iron, with the carburization of the iron near the steel, introduces a modification of the metals that constitutes a kind of gradation from one metal to the other, preventing the existence of a too strongly-marked line of separation between the steel and iron, giving a layer of semi-steel between them.

This manipulation of iron and steel is accompanied by a chemical action similar to that which occurs in the manufacture of blister-steel, namely: the action of carbon, leaving the steel, on the iron encasing it, and the formation of carbonic oxide with the oxygen of the slag within the iron, which gas aids, by the immediate treatment of the pile with rolls, the separation and outflow of the slag, securing a greatly increased purification of the iron in the casing of the pile.

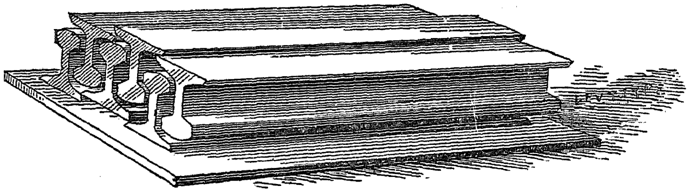
To avoid obscurity, the following descriptions are given of particular experiments made by this process, including, it will be seen, the manipulation of steel, varying from that which is too high in carbon, for application to structural uses, to that which is probably too low for economical treatment.

Several iron boxes, size 15 x 15 x 2 inches, internal dimensions, were made from common plate iron $\frac{3}{16}$th inch thick, and riveted at the sides. One of these was partly filled at the Pennsylvania Steel Works with metal from the Bessemer converter, after the carbon had been burnt out of the charge, and before the spiegeleisen was added. This partly-filled box, the open side being closed, was placed in the furnace and heated to a welding heat, when it was rolled into plate, of which a sample accompanies this paper. When the mass was reduced to about No. 6 gauge, the metal began to break from red-shortness, and continued to do so until it could no longer be passed through the rolls. As tested by Mr. Morrell, at the Cambria Works, it has carbon .035, and a tensile strength, in pounds to the square inch, with the grain of 62.900, and across the grain of 57.559; averaging 19 per cent. elongation, for length of sample, varying from 2.5 to 2.6 inches.

Another box was filled with rail steel, the same as poured into the ingot moulds, carbon .41; this steel blew off, in cooling, a large amount of gas, at the same time expelling a quantity of steel, which necessarily left the mass in a very porous condition. It was without the slightest difficulty rolled at a single heat into sound plate, gauge No. 11, like the sample, and is now on exhibition at the fair of the American Institute for inspection, showing conclusively that porosity of the steel does not embarrass the process or injure the product.

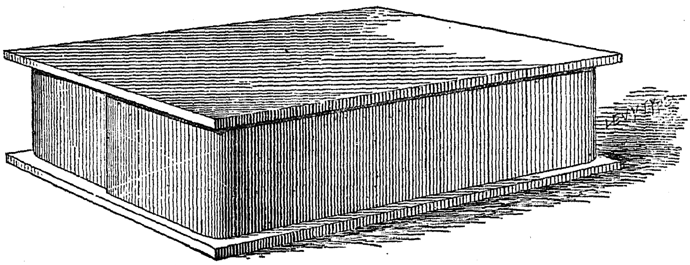
A box like those previously described, only made of plate of $\frac{3}{8}$th inch in thickness, was filled with steel like the last, and subsequently rolled into $\frac{1}{4}$-inch plate, of which a sample is presented. The reduction was too rapid, and the nearly white heat of the plate when finished, accounts for the appearance of the surface.

In another instance, sections of old steel rails cut to lengths of two feet were laid close together on a plate of muck-bar, alternating tops



and bottoms, and held in position by a band of muck-plate bent so as to form the four sides of a box six inches deep, the top of the box being another piece of muck-bar one inch thick and 15 x 28 inches dimensions, as shown in the sketch.

The steel in this box occupied less than half of the space within it, necessitating a reduction between the rolls of more than three inches before the metal could be compressed into a solid mass. No difficulty was experienced in rolling this pile into ¼-inch plate at a



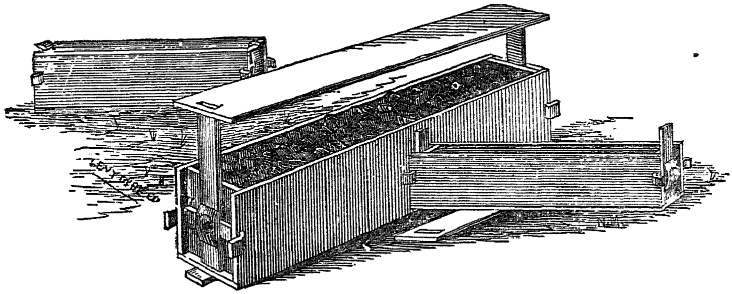
single heat. A sample is also presented of such a plate, and one of full size, with a duplicate of the box from which it was rolled, may be seen at the fair of the American Institute. It has tensile strength to the square inch of over 75,000 pounds, with a limit of elasticity for the same section of over 50,000 pounds.

A pile was made by placing together, compactly, forty-two plates of old steel springs which had lain in an uncovered scrap heap for months, or years, until covered with a thick coating of rust and dirt. The separate pieces were about one quarter of an inch in thickness, by three or three and a half inches in width. I speak from memory. They were boxed by using flat bars, made by breaking down old iron rails. The pile was about seven inches square and perhaps three feet in length. At the first heat it was rolled into blooms; at the second heat into billets for the wire mill. I have a sample for your inspection.

And finally I will add, that no difficulty has been experienced in reducing steel scrap, from any of its forms, to articles of commerce. I have here a piece of a bar, the steel in which was old files, and had it been necessary I could have presented for your inspection a much greater variety of samples.

The box for manipulating fine scrap is made of rectangular plates of iron of suitable thickness, fastened at the ends by ties, as shown in the sketch, which represents a box 32 inches long and about 6 inches square.

This box is made economically by machinery, and may be shipped in packages to the establishment where fine scrap is produced, where it can be put together and filled ready for the furnace before shipment. It is available and valuable for iron as well as steel, and should be the scrap-box of every machine shop.



I will not trespass further on your time by recounting other illustrations and experiments of the practical application of this process. These samples submitted will, I think, establish the fact that by this process a thoroughly satisfactory weld or union of the metals is secured, which meets the requirements of trade as well in respect to the extent and degree as in the economy of its application.

The product may be in the form of completed articles of commerce, such as bars, plates, tubes, etc., or it may be only in preparation for further treatment, as in the case of top plates for making piles for railroad rails, flat bars for cross-piling in making special grades of boiler plate, blooms, slabs, and shoot bars, segments for tubular iron, etc., etc.

The product varies of course with the quality of the steel, no one thinking that old steel rails will be transformed into steel of the highest quality, but it is claimed that the iron is materially improved by being worked over steel as the process requires; that the steel is not deteriorated by the process; that articles of iron-clad steel

are exempt from liability to fracture under concussive or vibratory strains to the degree that they would probably be if made of iron only; that the iron cover protects the steel from the corrosion to which it is liable from the presence of manganese; and that the manipulation to which steel is subject in this process alone effaces effectually all traces of porosity, seams, and surface cracks, all of which occasion or are in themselves incipient fractures.

It is believed that the product of this process, "combination," has exceptional advantages for use as a structural material as compared with either metal used alone. This special adaptation is illustrated by an experiment often made with an iron-clad bar of steel, which, with one end firmly fastened in a vise, is forcibly bent to and fro a great number of times, or struck repeatedly with a heavy hammer without showing any tendency to fracture. The ductile iron in the coating of the bar prevents the concentration at any one point of the vibratory strain put upon the outer surface of the steel, and is itself not affected injuriously by accidental scratches or indentations on its surface, as would be the case with bars or plates of naked steel treated in the same manner.

In the manufacture of railroad rails it seems feasible to attain, by this process, an ideal rail, combining enduring hardness in the head with toughness in the web and base—a union of homogeneity and fibre, in positions to yield the best result.

THE CHEMICAL COMPOSITION AND PHYSICAL PROPERTIES OF STEEL RAILS.

BY C. B. DUDLEY, PH.D., CHEMIST, PENNSYLVANIA RAILROAD CO., ALTOONA, PA.

In the spring of 1877, the Pennsylvania Railroad Company became so dissatisfied with the average life and wear of the steel rails it was then able to procure, that it determined to make an investigation into the chemical composition and physical properties of steel rails, with a view, first, to answering the question why one steel rail has to be removed from the track after, perhaps, eight months' service, while another lasts ten years; and, secondly, if this investigation succeeded in throwing light on this important subject, to use this information in securing better rails in the future. The results

of that investigation are contained in the following report. In presenting this report to the American Institute of Mining Engineers, which I am permitted to do by the kindness of the officers of the Pennsylvania Railroad Company, it has been thought best to retain the form and style in which the report was written. For although the report was originally written for the officers of the Pennsylvania Railroad Company, and, consequently, the discussion of details made a little more full than if it had been written as a scientific paper, yet this very fulness of detail may not be amiss in a field where knowledge is so urgently needed.

REPORT.

THEO. N. ELY, ESQ.,

Superintendent Motive Power, Pennsylvania Railroad Co.

DEAR SIR: I have finished the examination of the subject of steel rails in connection with their chemical constitution, physical properties, and wear, which has occupied my attention and study, more or less constantly, for six months past, and beg leave to present thereon the following report:

The question which has led to, and has followed in all its details, the investigation embraced in this report is: How shall the Pennsylvania Railroad Company obtain steel rails which shall give a satisfactory wear and be uniform in quality?

In order to understand the attempt which has been made to answer this question, it will be necessary to state a few preliminary principles. It seems to be agreed among metallurgists that the quality of a piece of steel depends upon two sets of circumstances: 1st. Its chemical composition; and, 2d. The treatment which the metal receives either during or after its manufacture. In other words, a piece of steel of certain chemical constitution and treatment will have certain physical qualities of strength, elasticity, ductility, power to resist wear, etc.; or, again, a different chemical constitution and treatment will give a piece of steel which will differ in one or more of these respects from the first; the former being possibly more valuable for rails, the latter for cutting-tools.

Now it is to be confessed at the outset that our knowledge of these two sets of circumstances, viz., the chemical constitution and treatment necessary to secure such a piece of steel as is desired, is far from being all that could be wished for. Nevertheless, some things are known, and the work which is described below is an at-

tempt to add to our knowledge in this respect, with regard to steel rails.

Now inasmuch as all our steel rails are made at present by the Bessemer or pneumatic process, and inasmuch as the conditions of successful working of this process are pretty well understood, it is assumed that the *treatment* which the steel receives during manufacture is constant or always alike. That this is an actual fact in practice is probably not true. The carelessness of workmen, and the want of proper appliances, may at times cause more or less variation in the treatment which the successive "blows" or heats in the Bessemer converter receive. These variations are, however, undoubtedly small, and where care is taken, and the Bessemer process given its full chance, the want of uniformity of product, so far as that uniformity depends upon treatment or method of manufacture, should be very small. I would not be understood as saying, however, that I think the Bessemer process has reached its full development, or is incapable of further improvement. How to obtain solid ingots, whether it is better to use the bottom or top cast, whether the slag is all separated before casting, at what temperature the rail should go through the rolls, and especially the last pass, and how to get this temperature, are questions still awaiting solution, and I think it undoubted that to one or more, or possibly all combined, of these uncertainties are due some of the anomalies which are often met with in steel.

Nevertheless, assuming, as has already been stated, that the Bessemer process, as at present understood and worked, is capable, in careful hands, of turning out a moderately uniform product, so far as that uniformity depends upon *treatment* or method of manufacture—which is undoubtedly the case—the question how to obtain a good rail becomes one as to the chemical composition of the steel, and this is the part of the problem to which I have devoted some labor and study. We are fortunately, however, not entirely dependent for uniformity of product, so far as that uniformity depends upon treatment or method of manufacture, upon the uniformity with which the rail manufacturers work the Bessemer process. Just here comes in the sphere for appropriate physical tests and inspection. For example, it seems clear to me that if a "blow" or heat in the Bessemer converter is badly treated at any point during the manufacture, so that the steel is spoiled, appropriate physical tests and inspection will reveal the fact, and thus enable us to protect ourselves by rejecting that "blow." So that the question with which

we started, viz., How shall the Pennsylvania Railroad Company obtain steel rails which shall give satisfactory wear and be uniform in quality? seems to be resolved into two others, viz.: (1.) With the present known metallurgical methods in the Bessemer process, what chemical composition shall the Pennsylvania Railroad Company prescribe for its rails? and (2.) What physical tests and inspection shall it apply to secure uniformity of product? What follows is an attempt to answer these questions.

1. CHEMICAL COMPOSITION.

It is well known that there are six impurities which exist in nearly all iron and steel in greater or less amount, which are known to have important influences upon their quality, even in small quantities. These are carbon, phosphorus, silicon, manganese, sulphur, and copper, and when we speak of the chemical composition of steel, we mean the amount of these various impurities which the steel contains. Steel, and especially pig iron, contain other impurities, such as titanium, cobalt, nickel, arsenic, etc., but these are commonly disregarded in analysis because their amount is usually very small, and some ot them are not known to have any influence upon the quality of the metal. A discussion as to the influence of these various impurities upon the quality of steel, will follow later. It is sufficient here to say, that almost the only effect that sulphur and copper are known to have on steel, is to render it what is technically known as "red short," that is, if a steel rail has too much sulphur and copper in it, it crushes in the rolls or flies to pieces, during manufacture. I am unable to find, anywhere, that sulphur and copper are said to have a deleterious effect on the wear or durability of a rail, and indeed, some metallurgists claim that they are advantageous in this respect. I have, therefore, not determined the sulphur or copper in the analyses given below, and would not recommend to prescribe any specifications in regard to them. We can safety trust the rail manufacturers not to give us rails containing too much sulphur and copper.

As to the other four impurities, the question now arises, how much of these various substances ought a good steel rail to contain? In order to answer this question, twenty-five pieces of steel rails have been carefully analyzed for carbon, phosphorus, silicon, and manganese. The borings for analysis were taken out of the physical test pieces described below. The chemical work was all done in duplicate. The carbon was determined by combustion, working upon 7 grams, dis-

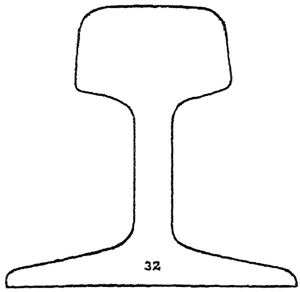
solving out the iron with solution of the double chloride of copper and ammonium, and burning the carbon with chromate of lead. The phosphorus was determined by the molybdate of ammonium method, working upon 3 grams, and dissolving the steel in aqua regia. The manganese was determined by the bromine method, separating the iron as basic acetate, and using acetate of soda as the precipitant. The silicon was determined in the usual way, working upon 10 grams, and dissolving in hydrochloric acid. The twenty-five samples of steel rail which were analyzed, have all been in actual service; some of them have broken in service, some have crushed in service, some have worn badly with short service, and some have endured long and hard service. The brands of steel represented in the series, are: Penn. steel, Cambria steel, Lackawanna Iron and Coal Co.'s steel, Cammel steel, Mersey steel, Ebbw Vale steel, and John Brown steel. It was thought that the series would represent, as fairly as a series of twenty-five samples could do, the actual results of good and bad service of rails on the Pennsylvania Railroad. Moreover, the samples were taken from all parts of the road, thereby rendering inoperative the influence of local causes upon the life and wear of the rails. If now the chemical composition of the good rails should show uniformity within narrow limits, while the chemical composition of the bad rails should likewise show uniformity; and if these uniformities of chemical composition in the good rails and bad rails should differ from each other, it would seem to be fair to conclude that the composition of the bad rails should be avoided, while that of the good rails should be adopted. That the good rails show a moderate uniformity of chemical composition, different from that of the bad rails, will be evident, I think, from an inspection of the analyses which follow.

One or two things more ought to be mentioned. In order to measure the value of a rail, whether good or bad, the approximate tonnage which has passed over each of these pieces of rail has been computed; it being of course evident to all that the burden which a rail has sustained, even though approximately determined, is a much more accurate measure of its value than time of service. Again, position in a track, whether on a curve or on a straight line, and also whether subject to high speed or not, obviously have an influence upon the durability of a rail, and attention has been given to these points in estimating the value of a rail. It will be noticed in the tables which follow that the principle of measuring the value of a rail by the tonnage which has passed over it has not been strictly followed. The rails have been divided in the tables into two classes

on this principle, viz.: *those which crushed or broke in service* and *those which did not crush or break in service.* The former are regarded as bad rails and the latter as good rails. This principle of division brings among the bad rails four whose tonnage is higher than the lowest tonnage of any rail among the good ones. But in view of the liability to accident which a broken or crushed rail may occasion, I think no one will claim that a rail which has broken or crushed in service should be classed among good rails, even though its tonnage may entitle it to be so rated.

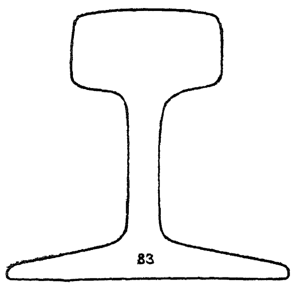
Finally, as a means of determining whether good rails differed from bad rails in physical qualities, such as tensile strength, ductility, etc., and if it was found that they did so differ, as a means of enabling us to specify what physical tests the steel for our rails ought to stand, careful physical tests have been made of every piece of steel analyzed except two, the samples sent for analysis of these two being so small as to prevent physical tests being made. The physical tests were made on Prof. Thurston's Torsional Testing Machine, as being the best means of determining the largest number of physical qualities at a single test. A copy of the diagrams obtained in making these tests accompanies this report, and will be referred to later.

The history of each piece of rail analyzed, together with an outline sketch of the piece (one-third size) as it appeared when removed from the track, the tonnage, chemical analysis, and results of physical tests are given in order below. Following these is a tabulated statement of these results, and the conclusions drawn from them:



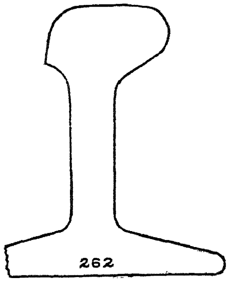
No. 32.—*Broken near Edgar Thomson Steel Works, first time train passed over it. Tonnage,* 0,000,000 *tons.*

Chemical Analysis.		Physical Tests.	
Carbon, ...	0.359	Angle of Torsion, ...	111°
Phosphorus, ...	.156	Moment of Torsion, ...	333
Manganese, ...	.505	Tensile Strength at Rupture, . . .	78,255
Silicon, ...	.035	" " " Elastic Limit, .	30,550
Total Hardeners, . .	1.055	Percentage of Elongation, ...	16.898
" in P. units, . .	39.4	Proportional Ultimate Resilience, .	29.80



No. 83.—*In service from June,* 1875, *to September,* 1876; *sixteen months. Was in south track on* 9° *curve, just east of Columbia Tunnel. Broke in service. Tonnage,* 10,027,131 *tons.*

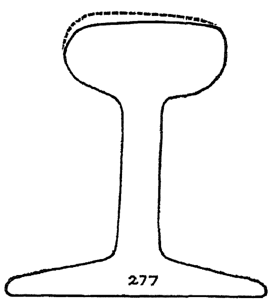
Chemical Analysis.		Physical Tests.	
Carbon, ...	0.323	Angle of Torsion, ...	106°
Phosphorus, ...	.135	Moment of Torsion, ...	340
Manganese, ...	.522	Tensile Strength at Rupture, . . .	79,900
Silicon, ...	.035	" " " Elastic Limit, .	33,135
Total Hardeners, . .	1.015	Percentage of Elongation, ...	15.509
" in P. units, . .	36.4	Proportional Ultimate Resilience, .	28.60



262

No. 262.—*In service from October,* 1868, *to November,* 1876; *eight years, one month. Was in south track on* 9° *curve, just west of Valley Creek Bridge. Tonnage,* 44,636,201 *tons.*

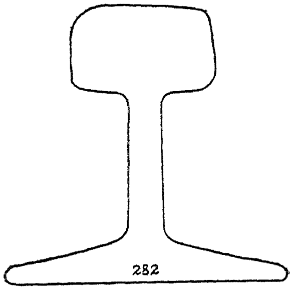
Chemical Analysis.		Physical Tests.
Carbon,	0.337	No physical tests were made, because of smallness of piece of rail sent.
Phosphorus,	.056	
Manganese,	.374	
Silicon,	.056	
Total Hardeners,	.823	
" in P. units,	27.1	



277

No. 277.—*In service from September,* 1872, *to December,* 1876; *four years, three months. Was in south track on tangent, about midway between South Elizabeth and Linden. Broke in service. Tonnage,* 16,600,728 *tons.*

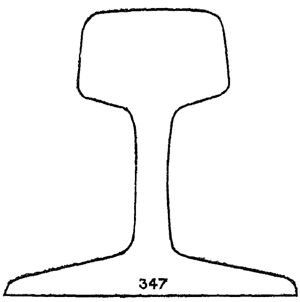
Chemical Analysis.		Physical Tests.	
Carbon,	0.573	Angle of Torsion,	101°
Phosphorus,	.075	Moment of Torsion,	433
Manganese,	.853	Tensile Strength at Rupture,	101,755
Silicon,	.182	" " " Elastic Limit,	43,005
Total Hardeners,	1.688	Percentage of Elongation,	14.169
" in P. units,	52.9	Proportional Ultimate Resilience,	36.81



282

No. 282.—*In service from August,* 1875, *to January,* 1877; *one year, five months. Was in single track near Marr's Run Station, on N. C. R. W. Broke in service. On tangent. Tonnage,* 4,535,318 *tons.*

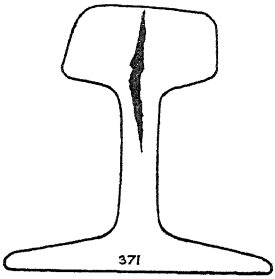
Chemical Analysis.		Physical Tests.
Carbon, ...	0.354	No physical tests were made, because of smallness of piece of rail sent.
Phosphorus, ...	.132	
Manganese, ...	.552	
Silicon, ...	.050	
Total Hardeners, . .	1.088	
" in P. units, . .	38.5	



347

No. 347.—*Broke after five days' service on New York Division. Tonnage,* 0,000,000 *tons.*

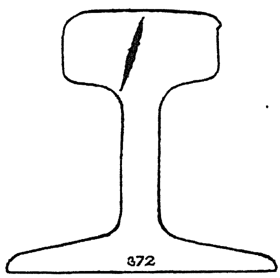
Chemical Analysis.		Physical Tests.	
Carbon, ...	0.387	Angle of Torsion, ...	67°
Phosphorus, ...	.056	Moment of Torsion, ...	306
Manganese, ...	.670	Tensile Strength at Rupture, . . .	71,910
Silicon, ...	.035	" " " Elastic Limit, .	30,550
Total Hardeners, . .	1.148	Percentage of Elongation, ...	6.467
" in P. units, . .	36.6	Proportional Ultimate Resilience, .	16.65



371

No. 371.—*In service from July,* 1876, *to March,* 1877; *eight months. Was in single track, on Tyrone and Clearfield Division, south of Mount Pleasant. On curve,* 16°. *Tonnage,* 2,741,056 *tons.*

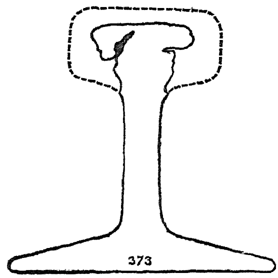
Chemical Analysis.		Physical Tests.	
Carbon, ...	0.368	Angle of Torsion, ...	85°
Phosphorus, ...	.127	Moment of Torsion, ...	342
Manganese, ...	.380	Tensile Strength at Rupture, . . .	80,370
Silicon, ...	.053	" " " Elastic Limit, .	47,000
Total Hardeners, . .	.946	Percentage of Elongation, ...	10.223
" in P. units, . .	35.8	Proportional Ultimate Resilience, .	24.68



372

No. 372.—*In service from July,* 1876, *to March,* 1877; *eight months. Was in single track on* 17° *curve, Tyrone and Clearfield Division, south of Mount Pleasant. Tonnage,* 2,741,056 *tons.*

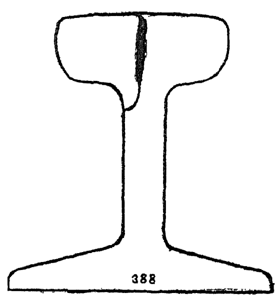
Chemical Analysis.		Physical Tests.	
Carbon, ...	0.416	Angle of Torsion, ...	102°
Phosphorus, ...	.155	Moment of Torsion, ...	346
Manganese, ...	.460	Tensile Strength at Rupture, . . .	81,310
Silicon, ...	.034	" " " Elastic Limit, .	30,550
Total Hardeners, . .	1.065	Percentage of Elongation, ...	14.433
" in P. units, . .	40.3	Proportional Ultimate Resilience, .	29.26



373

No. 373.—*In service from July,* 1876, *to March,* 1877; *eight months. Was in single track on* 20° *curve. Tyrone and Clearfield Division, south of Mount Pleasant. Tonnage,* 2,741,056 *tons.*

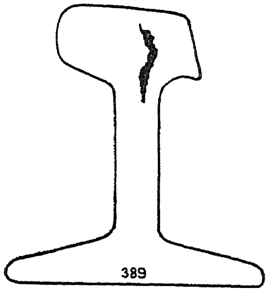
Chemical Analysis.		Physical Tests.	
Carbon,	0.300	Angle of Torsion,	102°
Phosphorus,	.138	Moment of Torsion,	281
Manganese,	.412	Tensile Strength at Rupture,	66,035
Silicon,	.024	" " " Elastic Limit,	25,850
Total Hardeners,	.874	Percentage of Elongation,	14.433
" in P. units,	33.2	Proportional Ultimate Resilience,	23.06



388

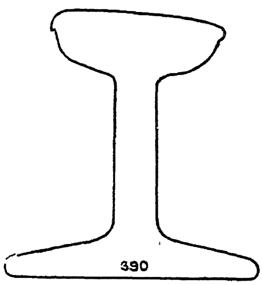
No. 388.—*In service from March,* 1867, *to March,* 1877; *ten years. Was in north track on a tangent near Ardmore Station. Tonnage,* 37,005,142 *tons.*

Chemical Analysis.		Physical Tests.	
Carbon,	0.303	Angle of Torsion,	120°
Phosphorus,	.166	Moment of Torsion,	322
Manganese,	.316	Tensile Strength at Rupture,	75,670
Silicon,	.032	" " " Elastic Limit,	31,725
Total Hardeners,	.817	Percentage of Elongation,	19.514
" in P. units,	34.6	Proportional Ultimate Resilience,	31.21



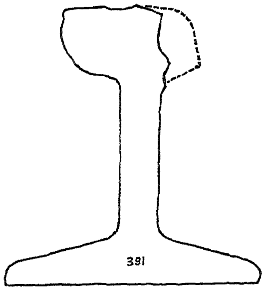
No. 389.—*In service from March,* 1872, *to April,* 1877; *five years, one month. Was on south track on a curve, one mile west of Huntingdon. Tonnage,* 34,333,639 *tons.*

CHEMICAL ANALYSIS.		PHYSICAL TESTS.	
Carbon,	0.343	Angle of Torsion,	121°
Phosphorus,	.127	Moment of Torsion,	320
Manganese,	.670	Tensile Strength at Rupture,	75,200
Silicon,	.036	" " " Elastic Limit,	30,550
Total Hardeners,	1.176	Percentage of Elongation,	19.813
" in P. units,	39.3	Proportional Ultimate Resilience,	31.02



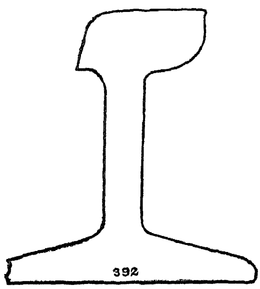
No. 390.—*In service from March,* 1868, *to March,* 1877; *nine years. Was in south track on* 2° *curve, on eastern slope of mountain. Pittsburgh Division. Tonnage,* 47,332,411 *tons.*

CHEMICAL ANALYSIS.		PHYSICAL TESTS.	
Carbon,	0.291	Angle of Torsion,	126°
Phosphorus,	.057	Moment of Torsion,	302
Manganese,	.354	Tensile Strength at Rupture,	70,970
Silicon,	.068	" " " Elastic Limit,	32,900
Total Hardeners,	.770	Percentage of Elongation,	21.337
" in P. units,	25.9	Proportional Ultimate Resilience,	31.95



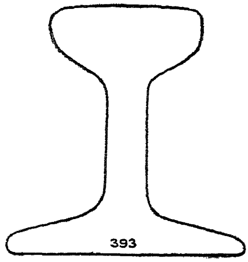
No. 391.—*In service from June,* 1871, *to April* 1877; *five years, ten months. Was in south track on* $4\frac{1}{2}°$ *curve,* 1700 *feet west of M. P.,* 84 *from Pittsburgh. Tonnage,* 30,873,173 *tons.*

Chemical Analysis.		Physical Tests.	
Carbon, ...	0.294	Angle of Torsion, ...	117°
Phosphorus, ...	.181	Moment of Torsion, ...	333
Manganese, ...	.354	Tensile Strength at Rupture, . . .	78,255
Silicon, ...	.020	" " " Elastic Limit, .	33,605
Total Hardeners, . .	.894	Percentage of Elongation, ...	18.626
" in P. units, . .	36.0	Proportional Ultimate Resilience, .	32.42



No. 392.—*In service from April,* 1871, *to April,* 1877; *six years. Was in south track on* 4° *curve,* 800 *feet east of M. P.,* 106 *from Pittsburgh. Tonnage,* 32,957,247 *tons.*

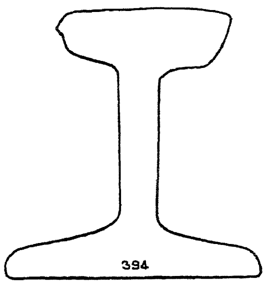
Chemical Analysis.		Physical Tests.	
Carbon, ...	0.231	Angle of Torsion, ...	151°
Phosphorus, ...	.087	Moment of Torsion, ...	294
Manganese, ...	.364	Tensile Strength at Rupture, . . .	69,090
Silicon, ...	.047	" " " Elastic Limit, .	30,550
Total Hardeners, . .	.729	Percentage of Elongation, ...	29.548
" in P. units, . .	26.0	Proportional Ultimate Resilience, .	37.24



393

No. 393.—*In service from July,* 1867, *to July,* 1873, *in north track at M. P.,* 155 *from Philadelphia; then from July,* 1873, *to April,* 1877, *in No.* 1, *south siding, Mifflin Yard. Total service, nine years, ten months. Tonnage,* 17,083,416 *tons.*

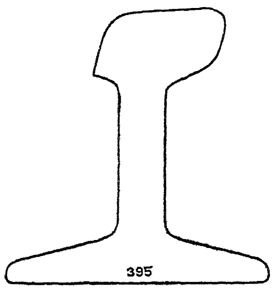
Chemical Analysis.		Physical Tests.	
Carbon,	0.219	Angle of Torsion,	217°
Phosphorus,	.065	Moment of Torsion,	285
Manganese,	.272	Tensile Strength at Rupture,	66,975
Silicon,	.028	" " " Elastic Limit,	28,200
Total Hardeners,	.584	Percentage of Elongation.	54.944
" in P. units,	20.6	Proportional Ultimate Resilience,	49.63



394

No. 394.—*In service from April,* 1871, *to April,* 1877; *six years. Was in south track on* 2° *curve,* 2600 *feet west of M. P.,* 27 *from Pittsburgh. Tonnage,* 25,043,350 *tons.*

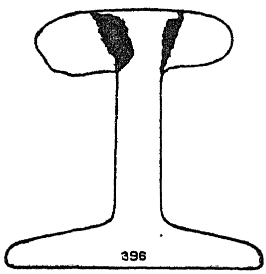
Chemical Analysis.		Physical Tests.	
Carbon,	0.286	Angle of Torsion,	149°
Phosphorus,	.083	Moment of Torsion,	322
Manganese,	.418	Tensile Strength at Rupture,	75,670
Silicon,	.023	" " " Elastic Limit,	45,825
Total Hardeners,	.810	Percentage of Elongation,	28.857
" in P. units,	27.3	Proportional Ultimate Resilience,	41.65



395

No. 395.—*In service from September,* 1872, *to March,* 1877; *four years, seven months. Was in south track on* 4° *curve,* 1200 *feet west of M. P.,* 59 *from Pittsburgh. Tonnage,* 24,606,889 *tons.*

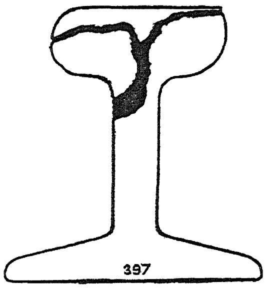
Chemical Analysis.		Physical Tests.	
Carbon,	0.353	Angle of Torsion,	134°
Phosphorus,	.103	Moment of Torsion,	338
Manganese,	.576	Tensile Strength at Rupture,	79,430
Silicon,	.059	" " " Elastic Limit,	32,900
Total Hardeners,	1.091	Percentage at Elongation,	23.860
" in P. units,	36.5	Proportional Ultimate Resilience,	37.49



396

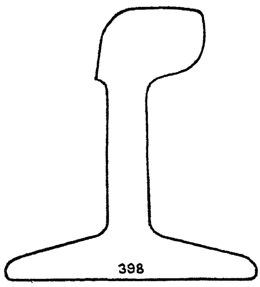
No. 396.—*In service from January,* 1874, *to January,* 1877; *three years. Was in Subdivision* 11, *Pittsburgh Division; the records of which were destroyed by the riots. Tonnage,* 13,683,266 *tons.*

Chemical Analysis.		Physical Tests.	
Carbon,	0.350	Angle of Torsion,	105°
Phosphorus,	.134	Moment of Torsion,	342
Manganese,	.626	Tensile Strength at Rupture,	80,370
Silicon,	.058	" " " Elastic Limit,	36,425
Total Hardeners,	1.168	Percentage of Elongation,	15.237
" in P. units,	40.5	Proportional Ultimate Resilience,	29.06


397

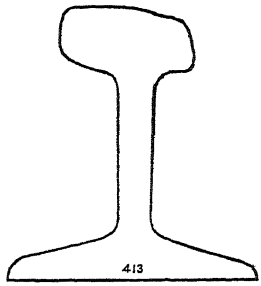
No. 397.—*In service from July,* 1872, *to May,* 1877; *four years, seven months. Was in south track on tangent* 650 *feet west of M. P.,* 11 *from Pittsburgh. Tonnage,* 21,935,613 *tons.*

CHEMICAL ANALYSIS.		PHYSICAL TESTS.	
Carbon,	0.365	Angle of Torsion,	82°
Phosphorus,	.130	Moment of Torsion,	260
Manganese,	.458	Tensile Strength at Rupture,	61,100
Silicon,	.020	" " " Elastic Limit,	25,850
Total Hardeners,	.973	Percentage of Elongation,	9.545
" in P. units,	35.3	Proportional Ultimate Resilience,	16.86


398

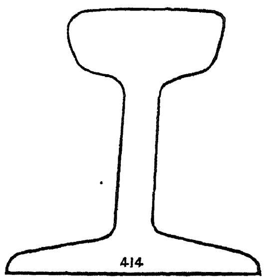
No. 398.—*In service from January,* 1871, *to March,* 1877; *six years, two months. Was in north track on* 4° *curve,* 120 *feet west of M. P.,* 12 *from Pittsburgh. Tonnage,* 27,296,043 *tons.*

CHEMICAL ANALYSIS.		PHYSICAL TESTS.	
Carbon,	0.225	Angle of Torsion,	130°
Phosphorus,	.111	Moment of Torsion,	282
Manganese,	.318	Tensile Strength at Rupture,	66,270
Silicon,	.016	" " " Elastic Limit,	27,025
Total Hardeners,	.670	Percentage of Elongation,	22.586
" in P. units,	25.8	Proportional Ultimate Resilience,	28.93



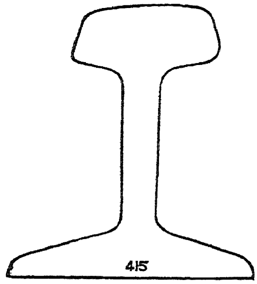
No. 413.—*In service from June,* 1867, *to October,* 1876; *nine years, four months. Was in single track on* 8° 40′ *curve, at west end of Schuylkill Bridge, Del. Ex. R. R. Tonnage,* 36,901,508 *tons.*

Chemical Analysis.		Physical Tests.	
Carbon,	0.233	Angle of Torsion,	175°
Phosphorus,	.041	Moment of Torsion,	280
Manganese,	.208	Tensile Strength at Rupture,	65,800
Silicon,	.074	" " " Elastic Limit,	28,435
Total Hardeners,	.556	Percentage of Elongation,	38.239
" in P. units,	19.7	Proportional Ultimate Resilience,	39.49



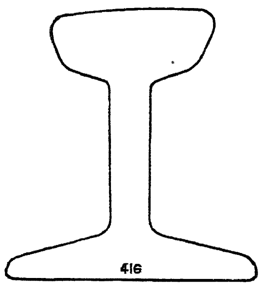
No. 414.—*In service from May,* 1867, *to November,* 1876; *nine years, six months. Was in west bound freight track on tangent, east of* 35*th Street Bridge, West Philadelphia. Tonnage,* 34,839,538 *tons.*

Chemical Analysis.		Physical Tests.	
Carbon,	0.309	Angle of Torsion,	148°
Phosphorus,	.058	Moment of Torsion,	292
Manganese,	.326	Tensile Strength at Rupture,	68,620
Silicon,	.030	" " " Elastic Limit,	27,730
Total Hardeners,	.723	Percentage of Elongation,	28.514
" in P. units,	24.1	Proportional Ultimate Resilience,	35.86



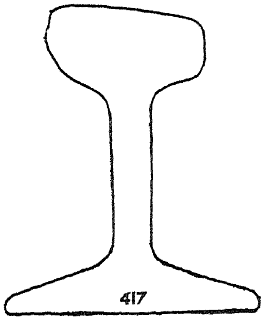
No. 415.—*In service from April,* 1867, *to July,* 1876; *nine years, three months. Was in south track, four years on* 2° *curve, and five years on tangent, near Marysville, Middle Division. Tonnage,* 48,037,879 *tons.*

Chemical Analysis.		Physical Tests.	
Carbon,	0.336	Angle of Torsion,	137°
Phosphorus,	.079	Moment of Torsion,	321
Manganese,	.458	Tensile Strength at Rupture,	75,435
Silicon,	.061	" " " Elastic Limit,	31,725
Total Hardeners,	.934	Percentage of Elongation,	24.833
" in P. units,	31.3	Proportional Ultimate Resilience,	36.75



No. 416.—*In service from June,* 1868, *to September,* 1876; *eight years, three months. Was in south track on tangent opposite Harrisburg Freight Warehouse. Tonnage,* 47,354,754 *tons.*

Chemical Analysis.		Physical Tests.	
Carbon,	0.283	Angle of Torsion,	127°
Phosphorus,	.114	Moment of Torsion,	289
Manganese,	.334	Tensile Strength at Rupture,	67,915
Silicon,	.030	" " " Elastic Limit,	28,200
Total Hardeners,	.761	Percentage of Elongation,	21.647
" in P. units,	29.0	Proportional Ultimate Resilience,	29.87



No. 417.—*In service from December,* 1867, *to July,* 1876; *nine years, six months. Was in north track on* $4\frac{1}{2}°$ *curve, at Jackstown Water Station, Middle Division. Tonnage,* 34,108,667 *tons.*

Chemical Analysis.		Physical Tests.	
Carbon,	0.345	Angle of Torsion,	124°
Phosphorus,	.075	Moment of Torsion,	312
Manganese,	.426	Tensile Strength at Rupture,	73,320
Silicon,	.041	" " " Elastic Limit,	30,080
Total Hardeners,	.887	Percentage of Elongation,	20.722
" in P. units,	29.6	Proportional Ultimate Resilience,	31.77

TABLE I.

Showing Tonnage, Location, and Results of Chemical Analysis of Twenty-five Samples of Steel Rails.

Rail No.	Tonnage.	Location.	C.	P.	Mn.	Si.	Total.	Tot. in Phos. Units.
415	48,037,879	2° C. and Tangent.	0.336	0.079	0.458	0.061	0.934	31.3
416	47,354,754	Tangent.	0.283	0.114	0.334	0.030	0.761	29.0
390	47,332,411	2° Curve.	0.291	0.057	0.354	0.068	0.770	25.9
262	44,636,201	9° "	0.337	0.056	0.374	0.056	0.823	27.1
413	36,901,508	8⅔° "	0.233	0.041	0.208	0.074	0.556	19.7
414	34,839,538	Tangent.	0.309	0.058	0.326	0.030	0.723	24.1
417	34,108,667	4½° Curve.	0.345	0.075	0.426	0.041	0.887	29.6
392	32,957,247	4° "	0.231	0.087	0.364	0.047	0.729	26.0
398	27,296,043	4° "	0.225	0.111	0.318	0.016	0.670	25.8
394	25,043,350	2° "	0.286	0.083	0.418	0.023	0.810	27.3
395	24,606,889	4° "	0.353	0.103	0.576	0.059	1.091	36.5
393	17,083,416	Tangent.	0.219	0.065	0.272	0.028	0.584	20.6
388	37,005,142	Tangent.	0.303	0.166	0.316	0.032	0.817	34.6
389	34,333,639	Curve.	0.343	0.127	0.670	0.036	1.176	39.3
391	30,873,173	4½° Curve.	0.294	0.181	0.354	0.020	0.849	36.0
397	21,935,613	Tangent.	0.365	0.130	0.458	0.020	0.973	35.3
277	16,600,728	"	0.573	0.075	0.853	0.182	1.688	52.9
396	13,683,266	Unknown.	0.350	0.134	0.626	0.058	1.168	40.5
83	10,027,131	9° Curve.	0.323	0.135	0.522	0.035	1.015	36.4
282	4,535,318	Tangent.	0.354	0.132	0.552	0.050	1.088	38.5
371	2,741,056	16° Curve.	0.386	0.127	0.380	0.053	0.946	35.8
372	2,741,056	17° "	0.416	0.155	0.460	0.034	1.065	40.3
373	2,741,056	20° "	0.300	0.138	0.412	0.024	0.874	33.2
347	5 Days' Service.	Unknown.	0.387	0.056	0.670	0.035	1.148	33.6
32	Broke 1st Train.	"	0.359	0.156	0.505	0.035	1.055	39.4

TABLE II.

Showing Tonnage, Location, and Results of Physical Tests of Twenty-five Samples of Steel Rails.

Rail No.	Tonnage.	Location.	Angle of Torsion.	Moment of Torsion.	Tensile Strength.	Tensile Strength at Elastic Limit.	Elongation.	Ultimate Resilience
415	48,037,879	2° C. and Tangent.	137°	321	75,435	31,725	0.248	36.75
416	47,354,754	Tangent.	127°	289	67,915	28,200	0.216	29.87
390	47,332,411	2° Curve.	126°	302	70,970	32,900	0.213	31.95
262	44,636,201	9° "	No	tests	made	of this	rail.	
413	36,901,508	8⅔° "	175°	280	65,800	28,435	0.382	39.49
414	34,839,538	Tangent.	148°	292	68,620	27,730	0.285	35.86
417	34,108,667	4½° Curve.	124°	312	73,320	30,080	0.207	31.77
392	32,957,247	4° "	151°	294	69,090	30,550	0.295	37.24
398	27,296,043	4° "	130°	282	66,270	27,025	0.226	28.93
394	25,043,350	2° "	149°	322	75,670	45,825	0.288	41.65
395	24,606,889	4° "	134°	338	79,430	32,900	0.239	37.49
393	17,083,416	Tangent.	217°	285	66,975	28,200	0.549	49.63
388	37,005,142	"	120°	322	75,670	31,725	0.195	31.21
389	34,333,639	Curve.	121°	320	75,200	30,550	0.198	31.02
391	30,873,173	4½° Curve.	117°	333	78,255	33,605	0.186	32.42
397	21,935,613	Tangent.	82°	260	61,100	25,850	0.095	16.86
277	16,600,728	"	101°	433	101,755	43,005	0.142	36.81
396	13,683,266	Unknown.	105°	342	80,370	36,425	0.152	29.06
83	10,027,131	9° Curve.	106°	340	79,900	33,135	0.155	28.60
282	4,535,318	Tangent.	No	tests	made	of this	rail.	
371	2,741,056	16° Curve.	85°	342	80,370	47,000	0.102	24.68
372	2,741,056	17° "	102°	346	81,310	30,550	0.144	29.26
373	2,741,056	20° "	102°	281	66,035	25,850	0.144	23.06
347	5 Days' Service.	Unknown.	67°	306	71,910	30,550	0.065	16.65
32	Broke 1st Train.	"	111°	333	78,255	30,550	0.169	29.80

TABLE III.

Showing Tonnage, Location, Results of Chemical Analysis, and Physical Tests of Twenty-five Samples of Steel Rails.

RAIL NO.	TONNAGE. Million Tons.	LOCATION.	C.	P.	MN.	SI.	TOTAL IN PHOS. UNITS.	TENSILE STRENGTH. 1000 lbs.	ELASTIC LIMIT. 1000 lbs.	PER CENT. OF ELONGATION	ULTIMATE RESILIENCE	
415	48	2° C. and Tangent.	.336	.079	.458	.061	31.3	75	32	25	37	Did not Break or Crush in Service.
416	47	Tangent.	.283	.114	.334	030	29.0	68	28	22	30	
390	47	2° Curve.	.291	.057	.354	.068	25.9	71	33	21	32	
262	45	9° "	.337	.056	.374	.056	27.1	No	tests	made		
413	37	$8\frac{2}{3}$° "	.233	.041	.208	.074	19.7	66	28	38	39	
414	35	Tangent.	.309	.058	.326	.030	24.1	69	28	28	36	
417	34	$4\frac{1}{2}$° Curve.	.345	.075	.426	.041	29.6	73	30	21	32	
392	33	4° "	.231	.087	.364	.047	26.0	69	30	29	37	
398	27	4° "	.225	.111	.318	.016	25.8	66	27	23	29	
394	25	2° "	.286	.083	.418	.023	27.3	75	46	29	42	
395	25	4° "	.353	.103	.576	.059	36.5	79	33	24	37	
393	17	Tangent.	.219	.065	.272	.028	20.6	67	28	55	50	
388	37	Tangent.	.303	.166	.316	.032	34.6	76	32	19	31	Broke or Crushed in Service.
389	34	Curve.	.343	.127	.670	.036	39.3	75	30	20	31	
391	31	$4\frac{1}{2}$° Curve.	.294	.181	.354	.020	36.0	78	34	19	32	
397	22	Tangent.	.365	.130	.458	.020	35.3	61	26	9	17	
277	17	"	.573	.075	.853	.182	52.9	101	43	14	37	
396	14	Unknown.	.350	.134	.626	.058	40.5	80	36	15	29	
83	10	9° Curve.	.323	.135	.522	.035	36.4	80	33	15	29	
282	5	Tangent.	.354	.132	.552	.050	38.5	No	tests	made		
371	3	16° Curve.	.386	.127	.380	.053	35.8	80	47	10	25	
372	3	17° "	.416	.155	.460	.034	40.3	81	30	14	29	
373	3	20° "	.300	.138	.412	.024	33.2	66	26	14	23	
347	0	Unknown.	.387	.056	.670	.035	33.6	72	30	6	17	
32	0	"	.359	.156	.505	.035	39.4	78	30	17	30	

Table III contains a condensed statement of the results of chemical analysis and physical tests, together with the tonnage and location of the twenty-five samples of rails analyzed. As has been previously mentioned, the rails analyzed have been divided into two groups. Those which *did not break or crush in service* have been placed first in Table III, and in the other tables of this report, and embrace the first twelve samples, down to and including No. 393. The remaining thirteen rails either *crushed or broke in service.* To a study of this statement attention is now directed. (See Plate III.)

But first a brief discussion as to the influence of the various substances affecting the quality of steel, viz., carbon, phosphorus, silicon, and manganese, upon the metal, will perhaps be in order. And here, at the start, I should like to frankly confess that our knowledge of the influence of these substances upon each other, and upon steel, is far from being as complete as we could wish. How thoroughly this lack of knowledge is recognized may be inferred from the fact that one of the important duties with which the United States Test Commission, organized some two years ago, was charged was this very point of the influence of the various impurities which exist in iron and steel upon the metal and upon each other in the metal. In view of this lack of knowledge, we can only, as it seems to me, apply what is already known, and, at the same time, study the results which we ourselves have obtained, with a view of deriving from them, as far as possible, the information they are calculated to teach. What, then, is the influence, so far as we know, of phosphorus, silicon, carbon, and manganese upon steel rails? Phosphorus, even in very small quantities, hardens steel and makes it brittle, and, at the same time, seems to render it especially liable to fracture from percussion or blows. Silicon hardens steel and renders it brittle, but in less degree than phosphorus. Carbon hardens steel and makes it brittle, and, at the same time, up to certain limits, adds to its strength, but seems to diminish its ductility or percentage of elongation almost directly in proportion to the increase in carbon and strength. Manganese hardens steel and renders it brittle, and adds to its strength, much like carbon, but in less degree, while, at the same time, it does not seem to diminish, as rapidly as carbon, the ductility, or percentage of elongation, proportional to the increase of manganese and strength.

Now it will be noticed that it is said of each of these four substances, phosphorus, silicon, carbon, and manganese, that they harden steel and render it brittle. Phosphorus and silicon seem to harden

steel without adding any other desirable qualities, except, perhaps, wear; while carbon and manganese seem likewise to have important influences upon the strength and ductility of the metal.

Leaving this point now for a moment, let us examine what qualities a steel rail needs to possess. A steel rail, as it seems to me, like every other piece of metal which is subject on the one hand to *strain*, and on the other to *abrasion* or *wear*, has two things to avoid. On the one hand, it must not be so hard and brittle as to break under the strain or blows to which it is to be subjected; while on the other, it must not be so soft as to yield too rapidly to the abrasive action which it is to be called upon to withstand. If, now, this reasoning be correct, and if the influence which we have ascribed to the carbon, phosphorus, manganese, etc., be such as they actually possess, it would seem that we ought to find, by a study of the results of the chemical analyses—since the series analyzed embrace rails which have been broken or crushed in service, as well as those which have endured long and hard service—I say it would seem that we ought to find what amount of these hardeners or brittle-makers is so great that the rails have a tendency to break or crush in service; and this point being known, it is obvious that to get the most satisfactory wear, it is only necessary to have the rails as hard as they can be made with safety. In other words, if the limit of hardeners or brittle-makers is known, it is obviously good policy to make our rails approach this limit as closely as possible.*

A word now as to the method of studying the hardeners. If we take any single hardener and follow it through the series, regarding it alone as to its influence on the steel, we will usually find that the principal thing that we will learn will be, that if any one of these hardeners is very high the rail broke in actual service. We will also learn that in the twelve rails which did not break or crush in service, the average of the carbon is 0.287 per cent., while in those which did break or crush in service, it is 0.366 per cent.; while in the eleven rails which withstood the highest tonnage, the average carbon is 0.30 per cent. Also, that in the twelve rails which did not break or crush in service, the average of the phosphorus is 0.077 per cent.; while in

* In connection with this paragraph, upon the relation between the hardness and wearing quality of steel, I would like to call attention to a more full discussion of this question in a paper in these Transactions under the title, "Does the Wearing Power of Steel Rails Increase with the Hardness of the Steel?" (p. 202). As will be seen there, the reasoning given above does not seem to be fully sustained by the facts in the case. (Oct. 12th, 1878.)

those rails which did break or crush in service, the average of the phosphorus is 0.132 per cent. And just here I may be permitted to call attention to what seems to me a very significant fact, viz., that in every case in which the phosphorus is above 0.12 per cent. the rail either broke or crushed in service. We will also learn by inspecting the analyses as to single elements, that the average manganese in the rails which did not crush or break is 0.369 per cent.; while the average in those which did crush or break is 0.521 per cent., and that the average silicon in the unbroken rails is 0.044 per cent., while in the broken or crushed rails it is 0.047 per cent.

But it seems to me that the true way to study the influence of the carbon, phosphorus, etc., upon steel is not to regard each one separately, but since all are known to render steel hard and brittle, to consider them *all as hardeners or brittle-makers*, and study them in connection with each other. There are two ways in which this may be done.

1. We may simply add together the percentages of carbon, phosphorus, silicon, and manganese, as they are given in the analyses, the sum obtained being regarded, of course, as a comparative measure of the hardness of the steel. Doing this, we find that the average sum of these constituents in those rails which did not break or crush in service amounts to 0.778 per cent.; while in those rails which did crush or break it is 1.030 per cent. It would, therefore, almost seem fair to conclude that we cannot with safety have the total sum of the carbon, phosphorus, silicon, and manganese in our rails as high as one per cent.

2. There is another way of looking at these results of analysis which seems to lead to even better results than are obtained by simply adding together the percentages of the hardeners. It is a question which has been somewhat discussed among metallurgists, how much carbon, for example, would have the same influence in rendering a steel hard and brittle as 0.01 per cent. of phosphorus. Or again, how much silicon or manganese would have the same influence in this respect as 0.01 per cent. of phosphorus. I am not aware that any definite relations have ever been discovered between these substances in this respect; but it is, of course, evident if we wish to express numerically the hardness of steel as derived from its chemical composition, we must estimate the influence of each of the hardeners in the same unit. Now I have assumed 0.01 per cent. of phosphorus as the unit of measurement, and have called this 0.01 per cent. a phosphorus unit. I have likewise assumed that 0.02 per cent. of

silicon, 0.03 per cent. of carbon, and 0.05 per cent. of manganese have each the same influence in rendering a steel hard and brittle as 0.01 per cent. of phosphorus. In any analysis of steel, therefore, the phosphorus units are found by adding together the phosphorus, $\frac{1}{2}$ the silicon, $\frac{1}{3}$ the carbon, and $\frac{1}{5}$ the manganese, expressed in hundredths per cent. Applying these data to the chemical analyses of the series we find that the average sum of the hardeners—expressed in phosphorus units—in the rails which did not break or crush in service is 27, while the average sum of those which did break or crush is 38. Examining now Table III a little in detail, we find that with one exception, No. 395, the total sum of the phosphorus units in the good rails in no case exceeds 31, while most of them are 29 or below; and when we examine those rails which either crushed or broke in service, in no case is the total sum of the phosphorus units less than 33. It would almost seem fair to conclude, therefore, that measured in phosphorus units, in the manner described, we cannot have rails whose total sum of hardeners in phosphorus units is over 31 or 32. Now, as has already been stated, I think no one will affirm that for successful wear the hardeners should be lower than is consistent with safety.* But in the rails which have not broken or crushed in service we find only one, No. 395, whose sum of hardeners is over 31. Therefore, measured in phosphorus units, it seems clear that the sum of the hardeners in rails for use on the Pennsylvania Railroad should not vary far from 30. The "total hardeners" and "phosphorus units" given in the pages of this report, which contain the diagrams of the rails analyzed, are obtained in the manner described above.

And now, how shall the hardeners be distributed? It has already been mentioned that in every case where the phosphorus is above 0.12 per cent., the rail either crushed or broke in service. Examining the phosphorus in rails which did not crush or break in service, we find that in no case is it above 0.11½ per cent., while in almost every case it is below 0.09 per cent. Remembering now that phosphorus makes steel brittle and especially liable to fracture from blows, and it would, perhaps, seem fair to place the limit of phosphorus at 0.10 per cent. As to silicon, the less in the rail the better. Nevertheless, as by the Bessemer process it is impossible to make rails entirely free from silicon, a small limit must be allowed for it. As the process is ordinarily worked, perhaps 0.04 per cent. will not be too high a limit.

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* See note to page 195.

The ability of rail-manufacturers to control the amount of carbon and manganese in the steel, requires a little broader limits for these two elements. If, now, we place the carbon at 0.25 per cent. to 0.35 per cent., and the manganese at from 0.30 per cent. to 0.40 per cent., and are able to obtain rails on this formula, which are not injured or spoiled during the manufacture, I think we will get rails which will be entirely safe, and, at the same time, give satisfactory wear. The sum of the hardeners, measured in phosphorus units, for the limits of the formula, amounts to 26 for the lower limits of carbon and manganese, and to 32 for the higher. In view of the tendency to higher carbon which has seemed to prevail for a few years past in rail manufacture, it may appear that the carbon limits given above are low. But that this is correct will, I think, be evident from the considerations which follow. Of course if we could make a formula just as we would like, it would, perhaps, be entirely scientific to take the average of the carbon, phosphorus, manganese, etc., in the best rails as the standard for this formula. Doing this for the rails which did not crush or break in service we have, carbon, 0.287 per cent.; phosphorus, 0.077 per cent.; manganese, 0.369 per cent.; silicon, 0.044 per cent.; and the same for the eleven rails which withstood the highest tonnage, we have, carbon, 0.30 per cent.; phosphorus, 0.091 per cent.; manganese, 0.38 per cent,; silicon, 0.045 per cent. But in view of the difficulty of obtaining low phosphorus in this country, we have put it as high perhaps as it should be, viz., 0.10 per cent. If, now, we make high carbon, we must diminish the manganese, or our total hardeners will be too high. That carbon should not be increased at the expense of manganese will, I think, be evident from an examination of the formula given just above, derived from the average composition of the best rails, as well as the separate analyses in Table III.

Both the formulas given above show the carbon to be lower than the manganese, and both are within the limits which we have given for carbon and manganese, viz., 0.25 per cent. to 0.35 per cent. for carbon, and 0.30 per cent. to 0.40 per cent. for manganese. Again, in the separate analyses of the rails which did not crush or break in service, in only one case, No. 413, is the carbon higher than the manganese. Moreover, if the influence which we have ascribed to manganese, viz., that it increases strength and hardness of steel without diminishing elongation as much as carbon would do, be correct, the rails which we get on a formula in which manganese is higher than carbon, will be less liable to break or crush in service,

and at the same time will, perhaps, give as satisfactory wear as if the carbon had been increased and the manganese diminished.

II. PHYSICAL TESTS AND INSPECTION.

The question now arises what physical tests and inspection shall the Pennsylvania Railroad Company prescribe to the rail manufacturers to enable it to secure rails uniform in quality?

The inspection which is at present employed seems to be amply sufficient to enable us to discard rails whose defects are evident to the eye. The question as to physical tests is not so easily answered. Nevertheless, three methods of applying physical tests suggest themselves.

1. The bending test now in use. It seems to me fatal to this test as at present conducted that the test bar, $\frac{3}{4}$ inch square by 12 inches long, is hammered out from a piece of the rail. It is conceded, I think, that in proper hands a piece of steel is changed by hammering. We are, therefore, not testing the steel in the rails, but a different quality of steel. If the present bending test is to be continued, I would suggest that the test-bar be cut from the rail head, instead of hammered from it.

2. The drop test. This test was in use for some time on the road to determine the quality and uniformity of the rails purchased. With regard to this test it may be said that unless the foundations are very solid, much of the force of the blow is lost; so that a rail tested under a rickety drop might stand the prescribed test, which, under a firm one would yield. It would seem, therefore, that the drop test, without extreme care and inspection on the part of the railroad company to hold the rail manufacturers up to specifications would give erroneous results. It is due to this reason, as I understand, that the drop test was abandoned.

3. Still another method of testing our rails suggests itself, and that is to ask the rail manufacturers to provide themselves with and use the same kind of machine upon which the physical tests of the series of rails analyzed have been made, viz., Thurston's Torsional Testing Machine. That this would be the most valuable method of securing uniformity in the rails furnished us, will be evident, it seems to me, from a study of the physical tests in Table III. It should be stated here that of all the data furnished by this machine, in regard to the physical qualities of metal (which data are given in full for record, in connection with the analyses, in the preceding pages, and are tabulated in Table II), we have selected out two, viz., tensile

strength and percentage of elongation, as best expressing the value of the steel with our present knowledge of the subject. It should also be stated that the tensile strength given in this report is $\frac{94}{100}$ of that obtained by the formula for determining the tensile strength which accompanies the machine, this being found by comparative tensile and torsion tests to most nearly express the tensile strength of this grade of steel.

Turning now our attention to Table III to the rails which crushed or broke in service, and noting that in the case of No. 397 the test is not very reliable on account of inability to secure a full-size test piece from the rail sent for analysis, we find that in every case except two, No. 373 and No. 347, the tensile strength of these rails is 75,000 pounds or above per square inch, while the percentage of elongation is 20 per cent. or below. Turning, now, to the rails which did not break or crush in service we find, with the single exception of No. 395, the tensile strength is between 65,000 pounds and 75,000 pounds per square inch, while the percentage of elongation is 21 per cent., or above. If, therefore, we had had specifications that our rails should have a tensile strength above 65,000 pounds per square inch, and a percentage of elongation above 20 per cent., as determined by the machine referred to, a simple inspection of Table III shows conclusively that we would have been able to reject every rail in this series which has crushed or broken in service. The same facts, and especially the defective elongation of the rails which broke or crushed in service, is made evident to the eye by an inspection of the accompanying diagrams of tests of the rails analyzed and described in this report, and from which the physical tests given in this report were computed.

In view of the discussion and considerations given above, it seems fair to conclude:

1. That with our present metallurgical methods high phosphorus in rails is inconsistent with safety.

2. That silicon should be as low as is consistent with the successful working of the Bessemer process.

3. That the best range for carbon is 0.25 per cent. to 0.35 per cent.

4. That the best range for manganese, all things considered, is from 0.30 per cent. to 0.40 per cent.

5. That the sum total of the hardeners, expressed in phosphorus units in the manner described, should not be above 31 or 32, nor below 25.

6. That the tensile strength of rails for use on the Pennsylvania Railroad, determined in the manner described in this report, should be above 65,000 pounds per square inch, and that the percentage of elongation determined in the same manner should be above 20 per cent.

I would, therefore, respectfully recommend that the following formula be prescribed for the chemical composition of rails for the use of the Pennsylvania Railroad, viz.:

Phosphorus, not above	0.10 per cent.
Silicon, not above	0.04 "
Carbon, between 0 25 and 0.35 per cent. with an aim at .	0 30 "
Manganese, between 0.30 and 0.40 per cent. with an aim at	0.35 "
Sulphur and copper, ...	No specifications.
All other impurities, not more than traces.	

Also, that the rail manufacturers be requested to procure one of Prof. R. H. Thurston's Torsional Testing Machines, and to furnish test pieces from each "blow," ready for testing to our Rail Inspector or other person authorized to attend to that work, who shall test the same on this machine, and that we shall be at liberty to reject all "blows" which do not conform to the physical tests, shown to be essential in the body of this report.

And I would also recommend that the rail manufacturers be informed that we feel ourselves at liberty, at any time, to make chemical analyses of the rails furnished us, and if the rails are found to differ from the above specifications, it will be regarded as a breach of contract and a proper subject for adjudication.

Very truly yours,

CHARLES B. DUDLEY,

ALTOONA, November 13th, 1877. Chemist, Penna. Railroad Co.

DOES THE WEARING POWER OF STEEL RAILS INCREASE WITH THE HARDNESS OF THE STEEL?

BY CHAS. B. DUDLEY, PH.D., CHEMIST, PENNSYLVANIA RAILROAD CO., ALTOONA, PA.

WHILE working, during the summer of 1877, upon the "Chemical Composition and Physical Properties of Steel Rails," the results of which are given in my report with this title, I was struck with the surprising wear which some of the rails, which would ordinarily be called soft rails, had endured. At that time I knew of no chemical measure of softness except low carbon, and I found that a number of rails with low carbon had endured as high or higher tonnage, with apparently as little loss of metal by wear, as those with higher carbon. My own work on steel rails that summer did not embrace any definite experiments as to the amount of metal worn off the rail per million tons which had passed over it; and so I could get no more definite answer from that work, to the question at the head of this paper, than was given by comparing the appearance of the worn section of the rail with its tonnage. This comparison, however, served to arouse in my mind the query, whether the commonly received opinion as to the relation between the hardness and wearing power of steel is correct, as applied to steel rails. This opinion, if I am right, is: the harder the steel, the better will be the wear, and the limit of hardness is simply one of safety; hard, brittle steel being, of course, more liable to break than soft, tough steel. The query, although aroused, did not bear any immediate fruit, and, as will be evident to any one reading it, the report above referred to was written with the commonly received opinion in mind. Since that time I have collected a little information upon this subject, which I should be glad to submit to the Institute, if for no other purpose, for the sake of arousing attention, and directing study to the question of hardness *versus* wear in steel rails.

Before making known this information, however, permit me a few words in reference to hardness. How shall we measure the hardness of steel? Of the various ways of getting indications as to the hardness of steel, which are known, three will serve our present purpose. These are: 1st. High carbon. It is generally agreed, I think, that at least within proper limits, the greater the amount of combined carbon in a piece of steel, the harder the steel, and I need not do

more than mention this fact to obtain your assent to it. 2d. The physical test of punching measures the hardness of steel. Data in regard to the wearing power of hard and soft rails, determined in this way, will be given below. 3d. The sum of the phosphorus units in a piece of steel measures its hardness. Phosphorus units, as is fully described in my report above referred to, are an attempt to measure the hardness of steel by estimating the combined hardening power of the phosphorus, silicon, carbon, and manganese in a piece of steel, in terms of the phosphorus. Now, by measuring hardness in these three ways, I have been able to collect the following information with regard to the relation between the hardness and wearing power of steel rails.

1. Some two years ago the Pennsylvania Railroad Company, in view of the unsatisfactory wear it was obtaining from steel rails, asked to have more carbon put into its rails, with a view of making them harder, to resist wear. Before the increase, the limits of carbon for rails to be used on the Pennsylvania Railroad was from 0.30 to 0.50 per cent. After the increase, the limits were from 0.40 to 0.50 per cent., thus securing on the average perhaps about a tenth of a per cent. more carbon in the steel. Now, Mr. W. H. Brown, Chief Engineer, Maintenance of Way, Pennsylvania Railroad, informs me that these rails of higher carbon are giving poorer wear than those made before the lower limit of carbon was raised. This opinion of Mr. Brown is based on his observation of the wear of these higher carbon rails, and on the number of renewals of these rails rendered necessary by the condition of the track.

2. Mr. J. T. Smith, General Manager of the Barrow Hæmatite Steel Works, England, read a paper on "Bessemer Steel Rails," before the Institution of Civil Engineers, in 1875.* The object of the paper was to show that Bessemer steel may be produced constant in quality, and that certain inexpensive tests may be applied, which shall determine the quality of the metal for railway purposes. The test proposed by Mr. Smith was to punch the fish-plate holes with a registering-press, the quality of the metal being judged of by the force required to punch the holes. This would render it possible to inspect and judge of the quality of every rail. I must refer you to Mr. Smith's paper for further information upon this point.

The point which is of especial interest to us now is that Mr. Smith examined thirty rails which had been eight years in service

* Proceedings Inst. Civil Eng., vol. xlii, p. 69.

on the main line of the Furness Railway. The rails were divided into two classes, on the basis of the force required to punch a hole $\frac{7}{8}$th of an inch in diameter, through the web $\frac{3}{4}$th of an inch thick. Twenty of the rails required for this purpose a force varying from $46\frac{1}{4}$ tons to $52\frac{1}{2}$tons, and were therefore called soft rails; while the remaining ten rails required for the same purpose a force varying from $56\frac{3}{4}$ tons to $82\frac{1}{2}$ tons, and were therefore called hard rails. The average force required to punch the soft rails was about 49 tons, while for the hard rails this average force was about $64\frac{3}{4}$ tons. Mr. Smith likewise gives the determinations of the carbon in these rails. In the twenty soft rails the carbon varied from 0.28 to 0.32 per cent., or an average of 0.30 per cent., while in the hard rails the carbon varied from 0.36 to 0.57 per cent., with an average of 0.44 per cent. Now as to the wear of these rails. The wear seems to have been determined by taking the difference between the original weight of the rail per yard and the weight per yard of the worn rail, and then reducing this to the percentage of the metal worn off. In the twenty soft rails, this percentage of wear varied from 10.38 to 16.24 per cent., with an average wear of 13.54 per cent., while in the ten hard rails the percentage of wear varied from 12 to 20.53 per cent., with an average of 15.18 per cent. These figures seem to me very significant, and to warrant Mr. Smith in the conclusion which he expresses, viz.: "Contrary to what might have been anticipated, greater hardness has not conduced to the longevity of the rails, and the softer ones show the minimum of wear."

3. With regard to the wear of rails, in which the hardness is measured in phosphorus units. On May 23d, 1876, Mr. R. Price Williams read a paper before the Institution of Civil Engineers, on "The Permanent Way of Railways."* In his investigation into the subject of steel rails, Mr. Williams found such a surprising difference in the wear of cetain rails which were side by side, and therefore subjected to the same traffic, that he had seven of these rails from the Great Northern Railway analyzed by Mr. Edward Riley. The results as to wear are given in number of million tons of traffic per $\frac{1}{16}$ inch worn off the rail. Four of these seven rails, measured in phosphorus units, sum up 31, 30, 32, and 25, and may therefore be called, in comparison with the others, soft rails. The remaining three rails sum up, in phosphorus units, 38, 40, and 47, and may be called hard rails. The rails are numbered, in Mr. Williams's series,

* Proceedings Inst. Civil Eng., vol. xlvi, p. 147.

Nos. 9, 17, 18, 21, 22, 23, and 24. Nos. 17 and 18 were side by side, and subjected to the same traffic. The phosphorus units in No. 17 are 38, and in No. 18, 30, one hard and one soft, as is seen. The hard rail withstood 5,251,000 ton per $\frac{1}{16}$ inch wear, while the soft one withstood 8,402,000 tons per $\frac{1}{16}$ inch wear. Again, Nos. 23 and 24 were side by side. The phosphorus units in No. 23 were 47, and in No. 24, 25, one hard and one soft, as before. The hard rail withstood 15,531,000 tons per $\frac{1}{16}$ inch wear, while the soft rail withstood 31,061,000 tons per $\frac{1}{16}$ inch wear. In Nos. 21 and 22, which were side by side, the hard rail shows a little the best wear, the figures being 9,283,000 tons per $\frac{1}{16}$ inch wear, for the hard rail, and 7,676,000 tons per $\frac{1}{16}$ inch wear for the soft rail. If now we take the average of the tonnage per $\frac{1}{16}$ inch wear for the three hard rails and the four soft ones, the result becomes quite striking. The four soft rails withstood an average tonnage of 15,567,000 tons per $\frac{1}{16}$ inch wear, while the three hard rails withstood, on the average, only 10,055,000 tons per $\frac{1}{16}$ inch of the metal worn off. The chemical composition of the one rail of this series, which withstood the highest tonnage, viz., 31,061,000 tons per $\frac{1}{16}$ inch wear, is so remarkable that I cannot forbear quoting the full analysis. This rail sums up 25 in phosphorus units, and contains: carbon, 0.270 per cent.; phosphorus, 0.100 per cent.; silicon, 0.020 per cent.; manganese, 0.259 per cent.; sulphur, 0.051 per cent.; copper, 0.025 per cent.; and iron, 99.475 per cent.

If enough has been said on this subject to direct attention to it, my object will have been accomplished. It is perhaps too soon to venture conclusions. The indications would seem to be, however, that under the conditions of wear to which a steel rail is subjected, viz., rolling friction, unlubricated surfaces, and great weight with small bearing surface, the quality of the metal neccessary to most successfully withstand the disintegrating forces, is best expressed by the word toughness, and not by hardness.

ALTOONA, Pa., Oct. 12th, 1878.

THE WATER SUPPLY AT THE BESSEMER STEEL WORKS OF THE EDGAR THOMSON STEEL COMPANY, LIMITED, PITTSBURGH, PENNA.

BY P. BARNES, SPRINGFIELD, ILLINOIS.

(Resident Engineer, 1873–75.)

SEVERAL statements have been made to the Institute, somewhat detached from each other, as to the cost of some parts of these works, but they have not included any extended description of the buildings or machinery. The general plan of the work connected with the water supply differs, in some important details, from that which has been common either in the Pittsburgh district or elsewhere, and a somewhat extended description may be of interest to the Institute, in connection with a note of the cost of the work.

The plan as a whole, and nearly all the important details, were developed by Mr. William P. Shinn, C.E., the general manager of the Company. The immediate and responsible charge of the work was in the hands of Mr. John C. Lewis, C.E., to whom had been committed all the work of this general description, including the laying out of the whole of the foundations, the erection of the buildings, and the grading and laying down of the entire railroad system. The continuous operation of the machinery and other fixtures of these waterworks, for more than three years, has fully demonstrated the correctness and economy of the plan adopted. No casualties of any moment have befallen the apparatus, and no improvements have been suggested as important except that the gate at the river end of the conduit should have been made more completely water-tight, so that the whole length of the conduit could be pumped out dry without resort to any temporary dam in the conduit itself.

In general terms the works of the Edgar Thomson Company are situated on a plateau, about 52 feet above the low-water level of the Monongahela River, and about 2000 feet from its bank. The site of the works is bordered on one side by Turtle Creek, a small stream which drains a considerable area of coal mining territory. This creek is about 100 feet from the group of buildings which have been erected, but it flows into the Monongahela River at a point, about 1200 feet further up than that selected for the entrance to the waterworks conduit. It was ascertained by analysis that the water in the creek contains an important percentage of sulphate of lime, while the water of the river is of unusual purity, in respect of any salts

that would cause incrustation or corrosion in the boilers or other parts of the machinery. It was, therefore, determined to go to the river-bank for the water supply, and also to make in the conduit, at a convenient point close to the works, a side opening through which a secondary supply could be obtained from Turtle Creek, if at any time circumstances should seem to require it. This side opening was made, but the connection to the creek has never been finished, or brought in any way into use.

It was also considered that the distance was so great to the river-bank, some 2000 feet, that any machinery erected there would be entirely cut off from a direct connection with the other machinery or the main battery of boilers at the works, and hence it would have been an additional expense for attendance and for general maintenance. It was, therefore, determined to erect the pumping machinery in a well very near to the principal boiler-house and the other buildings, and also that the water should be led to this pump-well by its own gravitation, through a pipe or conduit laid at such a depth as should permit the entrance and the flow of the water from the river at its lowest recorded stages.

The use was thus required for the conduit of a simple glazed sewer pipe for the greater part of the distance, and of a plain red brick tunnel the rest of the way. This heavier tunnel was used for a distance of about 500 feet, at the inner end of the conduit, because of the probability that in the extension of the works, or of the yard, around the buildings, a depth of filling might eventually be laid over it greater than the strength of the glazed pipe would bear. It became necessary, therefore, to erect at the river-bank only such piers or bulkheads as should be sufficient to inclose and guard the entrance to the conduit against the common contingencies of sand or mud, of logs and driftwood generally, and also the more serious possibility of ice, and of injury from passing steamboats and coal barges.

It was found upon examination that there had always been a slight deflection of the current of the river toward the bank at the point fixed upon for the river pier, and that it had served to prevent any deposit there of loam or sand. There had also been found an old, abandoned log pier, in fair state of preservation below the water-level, and the spot selected for the entrance pier was fixed upon chiefly on account of the protection afforded to it by the old pier from drift-wood, the new pier being placed close below the old one. Another reason for this location, and a more important one, was the fact that the old pier had been known for years, and hence avoided,

by all the river pilots, and this fact quite fully insured the new pier against any damage from the ordinary navigation of the river.

The entrance pier to the conduit was built on a concrete bed laid in a small coffer dam in about 8 feet of water. No special difficulty was experienced in any of the river work other than that incidental to the occurrence of logs and rocks on the bottom. The pier itself is a hollow block of cut stone masonry, 2 feet square in the clear inside, and 18 inches thick. Each course was made up of stones 18 inches wide, 18 inches high, and 3 feet 6 inches long, and each course was set so as to break joints with the course beneath it. On the top of the pier was laid, and bolted down, a heavy cast-iron plate with a hinged door or lid by which access can be had to the interior.

On the down-stream side of this pier is an entrance to the central well, covered with a grating to exclude drift-wood. On the bank side of the well a heavy iron gate was set in an iron frame so as to close the entrance to the conduit. The office of this gate is to shut out the disturbed water at the bottom of the river in a time of flood. As already noted, it has been found that this gate should have been more accurately fitted, so that by it the conduit could be closed entirely from the river.

The mouth of the conduit is set in this pier at a depth of 2 feet below the usual summer level of the river, and hence deep enough to meet all contingencies of lower water except the emptying of the slack-water pool of the river upon which the property of the Company is situated. It was found that this would occur but very rarely, and never without an ample notice, and also that it would never continue for more than two or three days at a time. Hence it was determined, after making a careful estimate of the cost of supplying the conduit by a temporary pump during such an emptying of the pool, to fix the entrance to it at 2 feet below the summer low-water level.

A fall of 1 inch per 100 feet was fixed upon for the conduit toward the works, and the pipes were laid in a coffer-dam extending from the river pier to the shore. This distance is 120 feet, and as the outer end of the conduit is 3 feet above the bottom of the river the pipe was laid on a wall of rubble masonry, and covered with a mass of the same material. The river pier stands at a distance of 150 feet from the edge of the beach, which, at this point, is about 30 feet wide, and reaches back to a sand and gravel bluff, rising quite abruptly to a height of 15 feet. From this bluff the

surface of the ground continues nearly level for a distance of 1200 feet toward the works.

In the face or edge of this bluff, or bank, it was determined to build a second pier, in which suitable screens could be put to keep back all leaves or chips that might find their way through the grating at the river pier. It was also considered needful to provide thus a means of admitting the water to the conduit in the high stages of the river, from a point near the surface of the water. Hence two openings were put in the face of this bank pier, and a gate or door was put on the lower one, so that the water could be shut out of the pier when it had risen high enough to flow in at the upper opening. This pier is 19 feet long and 21 feet deep from the river face. It is set with its outer face flush with the line of the bluff, which at this point had remained unchanged for many years. In it are two wells, 4 by 6 feet, and separated from each other by a 24-inch brick wall, which extends the whole height of the pier. On each face of this wall is fitted a grooved iron rail by which a screen-box is guided from the top to the bottom of the well. The water enters the outer well from the river pier, through the 20-inch pipe referred to, and passing through one or the other of the screens (one only being in use at a time) upon the intermediate wall, it makes its way into the principal line of the conduit. This bank pier is set on a hard gravel bed with a simple timber platform beneath it. It is built of heavy bridge masonry, and is carried up three feet above the surface of the adjacent ground, far enough to be always accessible even during very high water.

The main line of the conduit was laid through a bed of blue clay and gravel, nearly the whole way to the pumping well at the works, at an average depth of 20 feet below the surface. Shafts were sunk about 25 feet apart along the line, and between them, at the required depth, a small tunnel was driven just large enough to admit of the convenient laying in it of the 20-inch glazed pipe. These pipes were furnished in 2-feet lengths, and were bedded carefully in the gravel formation of the tunnel, the joints being filled with cement and pointed on the outside. Gravel was then rammed solid around the pipes in the tunnel and on top of them in the shafts.

At distances of 400 feet along this part of the conduit brick manholes were built in the shafts, extending to the surface, so that convenient access may be had at any time to any part of the conduit. It is believed that this part of the work has suffered no settling or impairment of any kind. These man-hole shafts were carried 3 feet

deeper than the grade of the conduit, so that they serve to a certain extent as silt basins, and they can be readily cleaned out from the surface if required. A heavy iron-coping plate with a lid was put on each of them.

At a point 1450 feet from the river, the conduit passes under a very heavy bank, upon which the Pittsburgh Division of the Baltimore and Ohio Railroad passes through the property. It was determined that beneath this bank and the rest of the way to the pump-well, a distance of 300 feet, the conduit should be built of brick. This rendered it needful to enlarge considerably the size of the tunnel driven between the working shafts, and as the nature of the ground changed somewhat, and still more on account of the extremely wet and stormy weather during which this part of the work had to be done, some slips and caving in of the ground occurred. This caused some delay, and part of the brick arching had to be finished in an open cutting. It is supposed that there may be cracks or leaks in this part of the work, but as no possible opportunity has been found for a close examination of the conduit since the work was finished in December, 1874, it is not clearly known where the leaks are.

The well in which the pumps are placed is 1750 feet from the river, and 150 feet from the boiler-house of the works. It is 12 feet clear diameter, and was sunk 26 feet through a firm gravel, and 6 feet into the shaly rock of the region. The brick conduit enters it at a distance of 4 feet above the bottom, so that a large silt basin is thus provided. The well is lined with a 16-inch brick wall to a point 35 feet below the general level of the yard of the works, or to a height of 20 feet above extreme low water in the river. In the side of this wall an arch was built, through which it was proposed to make a connection to Turtle Creek, at a distance of 175 feet, but as already stated, this has not yet been deemed needful.

The height of this lining wall was fixed by the maximum distance, 22 feet, through which it was considered expedient that the pumps should be required to lift the water under any circumstances. At this level, therefore, a heavy frame of iron beams was set to sustain the pumps, and an inclosing iron shell or tank 14 feet square. The object of this tank is to prevent the submerging of the pumps even in an extremely high stage of the river, and as the water sometimes rises 28 feet above the usual summer level, it was needful to make the shell 10 feet deep. It is set on the beams, and anchored to them by bolts, the beams themselves reaching out over the wall, and

secured against the tendency of the shell to float in time of high water by the weight of the inclosing side-walls built above them.

A wide concrete base was laid around the top of the brick well-lining, and on this a substantial rubble wall was laid, inclosing the iron shell, and to this wall the shell is anchored laterally, so that it is secured against collapsing from the external pressure of the water in time of flood. The side-walls are to be carried up to the general level of the works, and are now covered in with a suitable inclosing house.

It will be understood then, that the pumps are placed in a water-tight shell or tank, in an inclosed pit, 14 feet square and 20 feet deep, and also that the well extends 28 feet below the pumps. A water-tight manhole plate is set in the bottom of the shell, and access is thus provided to the well beneath, to the foot-valves of the pumps, and also to the inner end of the conduit. A substantial stairway gives access to the pumps from the yard level, the whole being arranged for convenience of access and for repair.

There are two pumps of the Worthington duplex type with 16-inch steam cylinders, 14-inch water cylinders, and 12-inch stroke. They are bolted to the floor of the iron shell, and to the iron frame beneath it, and are fitted with all the usual appliances for convenience of working, including foot-valves and strainers in the well beneath them.

A common cast-iron pipe is led from the pumps to the water-tank, a distance of 350 feet, and to this pipe either or both of the pumps may be connected at pleasure, separate gate-valves being provided for the purpose. This pipe is carried vertically upward from the pumps to a point 4 feet beneath the yard level, and it then runs to the boiler-house and through it to the water-tank.

The steam supply for these water-pumps is taken, as already stated, from the main battery of boilers and is carried to them by a 4-inch pipe above ground. A regulating valve is placed in this pipe, just inside the boiler-house, and it is automatically and very completely controlled by a float set in a small secondary tank, the water-level varying in this tank with that in the large storage-tank 200 feet distant, through a connecting-pipe.

This large tank is of the ordinary railroad-station type, and of a capacity of 20,000 gallons. It is 20 feet diameter and 12 feet deep, and is set on heavy posts with stone foundations. It is inclosed in a suitable house, and is heated in winter by a common stove.

The delivery pipe from the pump is carried up over the upper

edge of the tank, and the supply pipes for the pumps and boilers are taken out at the bottom. A series of wire screens was provided to keep back the small drift-stuff that might find its way through the other screens and strainers that had been fixed in the conduit. The pipes leading out at the bottom of the tank were all guarded against the flow into them of the mud that would settle in time of flood on the bottom.

It will thus be seen that this water-works scheme differs from others that have been devised for the same general purpose, and under similar circumstances, in the use of a large low-priced pipe through which the water flows by gravitation to the pumps, in place of a smaller iron pipe or force main. The pumps are placed 8 feet below the extreme high-water mark, being set in a water-tight inclosure, so as to bring them within a reasonable limit of lifting distance above the low-water level.

The chief advantage of the location of the pumps at the works, instead of at the river-bank, 2000 feet distant, obviously lies in the economy of the steam supply and the attendance.

The following is a classified account of the cost of the work:

No.	Class.	Amount.
1	Cement,	$859
2	Sand,	71
3	Stone masonry,	5054
4	Red brick,	1142
5	Sewer-pipe for conduit,	1408
6	Brickwork in pump-well,	418
7	Iron tank, inclosing pumps,	1254
8	Duplex pumps,	3467
9	Cast-iron pipe,	845
10	Water tank (R.R. pattern),	820
11	Temporary steam supply,	170
12	Coal,	174
13	Lumber,	1558
14	Castings,	349
15	Hardware,	253
16	Iron beams,	737
17	Conduit, contract for laying,	6878
18	Conduit, day work,	283
19	Common labor,	2012
20	Skilled labor,	2134
		$29,886

NOTE UPON A PECULIAR VARIETY OF ANTHRACITE.

BY ECKLEY B. COXE, DRIFTON LUZERNE CO., PA.

I WISH to call the attention of the Institute to a peculiar variety of anthracite which occurs in the Buck Mountain vein at our collieries at Drifton, and in the same and other veins in different localities in the anthracite coal-fields. It is known among the workmen as iron-gray or cast-iron. It has a dull, greasy appearance, as if a piece of ordinary anthracite had been rubbed with a dirty and greasy rag and allowed to dry. It was formerly considered a very impure coal containing a high percentage of ash, and was picked out of the coal and thrown away. But the following analysis of it and of its ash, made by Mr. J. Blodget Britton, of Philadelphia, shows that such is not the case, the percentage of ash not being much higher than that of the ordinary anthracites as they are prepared for market.

ANALYSIS OF COAL.

Water,	4.36	Fixed carbon, by difference,	84.90
Volatile combustible matter,	3 48		
Ash,	7.26		100.00

The carbon proved to some extent to be of the nature of graphite, and slowly combustible.

ANALYSIS OF ASH.

Silica,	25.66	Sulphur,	.17
Alumina,	27.03	Phosphoric acid,	.21
Ferric oxide,	42.83	Alkali, undetermined matter and loss,	.60
Lime,	1.56		
Magnesia,	1.83		
Manganous oxide,	.11		100.00

We recently collected a quantity of the substance and made a fire with it, in an ordinary stove, which was kept up several days. We found that it burned with intense heat, and apparently as freely as our other coal does. Mr. Britton is probably right in his view, that the difference in appearance is not due to impurities in the coal, but to the fact that the carbon is in a more or less graphitic form, as many of the standard coals are no purer.

I bring this before the Institute as I know that a large quantity of this valuable fuel is thrown away every year, on account of its supposed impurities.

PROCEEDINGS

OF THE

BALTIMORE MEETING,

FEBRUARY, 1879.

THE first session was held in the small hall of the Academy of Music, on Tuesday evening, February 18th, 1879.

The proceedings were opened by the reading, by President Eckley B. Coxe, of the following Address:

SECONDARY TECHNICAL EDUCATION.

Technical education is a subject to which both the American Institute of Mining Engineers and the American Society of Civil Engineers have of late years given much attention, and deservedly so. We all remember the admirable address of our former President, Mr. Holley, at the Washington meeting, upon this subject, and the very interesting reunion of the two societies in Philadelphia for its discussion. The problem to the solution of which the meeting devoted its attention was, "Shall the practical education of the engineer in the mine, in the rolling-mill, in the workshop, or in the field, precede, be contemporaneous with, or follow his theoretical studies in the technical school?" I do not propose to add anything to what was so admirably said by so many of our leading engineers on that occasion; but I shall ask your attention to another question in technical education, which seems to me equally worthy of our consideration, and which, I am sorry to say, has as yet attracted but little attention in this country. It is, "Shall the apprentices (I call them so for the want of a better general name), who are growing up and learning their trades in and around our mines and works, and from whose ranks the master mechanics and foremen of the future must be taken—shall they be provided with some means of obtaining as much theoretical training as may be necessary to enable them properly to understand the various processes and operations that they may be called upon to superintend, and not be obliged to pick up, as best they may, here a little and there a little truth mingled with much error?" In the former case, those who have the ability and industry to avail themselves of the advantages that should be offered to them would fit themselves to become real foremen, men who would understand and appreciate the true engineer and his methods. Of late years the young men of this country who have wished to enter our profession have had great facilities offered to them for so doing; there have been undoubtedly great improvements in the methods of instruction; and although I am very far from imagining that no further progress can be made, or is demanded, yet I feel sure that

those who in the future will be called upon to fill our places will be fully competent to do it. But nothing has as yet been done toward providing any means for what I call "*secondary technical education.*" In this, as in so many other things relating to education, Germany leads the van. Her schools for mining engineers are already more than a hundred years old, and enjoy a world-wide reputation, and for more than half a century she has had her "*Steigerschulen,*"—schools for master miners. In almost every important mineral district, the "*Steigerschule*" is a part of the administration of the mines, and in them those of the boys employed about the works, who desire to improve themselves, are instructed in all the branches of science which the practical experience of half a century has proved to be the most important for making good foremen. Similar schools exist in that country for the machinist and other trades. Some of those whom I see around me no doubt recall with pleasure the instruction they obtained from the very intelligent Steigers, graduates of such schools, with whom they came in contact when making their practical course of study, or when visiting the mines in Germany.

Promotion follows as in the army here, those who stand highest receive the best vacant positions, and all the foremen are taken from the graduates of the school, rising gradually from the less to the more important offices. The advantage to the engineer of having under him foremen who can make a drawing, and, therefore, can read and work from one; who know the chemical and physical properties of fire-damp, and consequently the reasons for the rules and regulations which must be enforced in a mine where it is known to exist; who understand the principles of combustion, and, therefore, why a furnace of one construction is better than another, and, why the directions given for firing or using them are essential, is patent to every one who has been obliged to erect or carry on works with foremen who, though intelligent and willing, were entirely ignorant of the principles involved in the operations they were superintending. The United States is entering upon a new epoch in her mining and metallurgical history. We have almost ceased to draw upon foreign countries for all the more important metals, with the exception of tin, of which as yet no workable deposits are known within our borders; but in iron (including steel), gold, silver, mercury, copper, lead, zinc, and nickel, we have become practically independent of the world: some we already ship to foreign countries, and of most of them we shall probably become exporters before many years

have rolled round. In other words, gentlemen, we must now prepare to meet the nations of Europe in the markets of the world with the products of our mines, our furnaces, our rolling-mills, and our factories, fully determined to obtain a firm foothold there for the sale of our surplus commodities; not hoping to monopolize them, but simply to secure and retain an independent position. We have many advantages,—a country of great resources, cheap land, cheap coal, cheap raw material, cheap food, intelligent workmen, plenty of room for development, and in many respects a good government, well suited to foster such industries; but we should not be satisfied with this. We should endeavor to strengthen our position by educating as well as possible the engineers who are to plan and superintend our works, by training both theoretically and practically the foremen who, coming from the ranks of the workmen, will act as a link between the school-trained engineer and the latter, understanding, as they will do, the feelings, the ideas, the aspirations, the strong points, and the weaknesses of those by whom the actual labor must be performed, and who form the basis of successful industry. With good engineers, with such foremen instructed sufficiently in theory to understand and carry out any process they may be put in charge of, having the respect not only of those over them, but also of the men under them, looking forward to their gradual advancement as their years of service and experience increase, and with intelligent workmen such as our country already has and will continue to produce, we can, by carefully utilizing all of our natural advantages, establish *mining* in that wide sense, in which it is understood by our Institute, as one of the surest, most enduring, and most important elements of the prosperity of our country.

To some, indeed, it may at first appear that the experiment is a dangerous one; that we are treading upon unknown and treacherous ground; that we may be sowing the seed of future trouble; that, instead of good foremen, we may only make discontented workmen; and they may quote to us Pope's well-known lines:

> "A little learning is a dangerous thing;
> Drink deep, or taste not of the Pierian spring."

But, gentlemen, while regarding Pope as a great poet and, in many respects, a keen observer of human nature, I must take issue with him here; for to me it seems that true knowledge, however small the amount may be, can never be dangerous. It were better, if we cannot drink deep, to put but one drop to our parched lips, rather than never to taste of its waters. It is *false* learning, be the amount

great or little, that, mingling with the truth, and corrupting it, causes the danger; and it is for the purpose of preventing the workmen from imbibing false ideas and opinions, so many of which they now drink in when acquiring practical knowledge, that I advocate the introduction of these schools. Educate the people, educate your foremen, educate your engineers, strive to give to every one some drops, however few, of the water of true knowledge, and we need not be anxious as to its effects, if it is pure, and contains no germs of corruption. The future, not only of our industries, but of our country itself, depends upon this; for no republic whose corner-stone is not the education of the masses can long continue to hold its place among the nations of the world, or even exist. And further, we may well inquire what, in engineering, is "*a little learning?*" Who of those that, like myself, commenced the study of the profession more than twenty years ago, and can recall the great changes that have taken place in the iron and steel industries, the improvement in the steam-engine, the creation and development of the petroleum industries, the complete uprooting of many of the laws of chemistry and physics as taught us, the changes in the views of geologists, the new methods of studying mineralogy and geology, opened up by the microscope, and the thousand other changes of greater or less importance in science—who of those, I say, looking forward to the development that will be sure to occur in the next twenty years, will dare to assert that he has more than "a little learning" in his own special branch, and that he has drunk deep of the Pierian spring? It is not the imparting of true knowledge to the workman that makes him unruly and discontented; it is by cutting him off from obtaining it, and forcing him to appease that thirst for knowledge, which exists in so many men, by listening to the uncontradicted assertion of the so-called working-men's friends, political demagogues, and other charlatans who suck the life-blood of the laboring class, living upon their savings without doing any real work themselves, that we sow the seeds whose fruits are seen in strikes and lockouts, and the riots, burning of factories, and violence which so often accompany them.

The great industrial nations of the continent of Europe are already in motion in the right direction. In all parts of Germany where mining is carried on to any extent, the *Steigerschulen* are to be found. France has her *Ecole des Mineurs* in the department of the Gard. Austria also has hers; and England, if she desires to maintain her industrial supremacy, must profit by their example.

This is a subject well worthy of the attention not only of the directors, managers, and stockholders of our railroads, machine-shops, mines, furnaces, and other industrial enterprises, but also of all who have any stake in the future of the country, or who are interested in the real welfare of the working classes. As man and boy, I have passed some portions of almost every one of the last forty years in the anthracite coal-fields of Pennsylvania. I have seen the production of that valuable fuel increased from less than one million to over twenty millions of tons per annum, and for the last fourteen years the greater part of my time has been spent in direct contact with the working miners. Looking back upon the coal regions as I first remember them, with the houses (if they deserved to be called by that name) without cellars, without plaster, with scarcely any privacy, with chimneys of wood, and with the out-houses, the pig-sties, the kitchen, and living-rooms so huddled together that it was hard to tell where one began and the other ended; with nothing for the miners to do but to work all day underground, and to pass their evenings in such houses; with nothing to look forward to for their children; and with whiskey-drinking as the only pleasure which was attainable, I cannot wonder that such seed should have brought forth such terrible fruit as the Mollie Maguire trials in Pennsylvania exposed to a horrified community. All parts of the civilized world are now more or less disturbed by the reports of the proceedings of the communists, the socialists, the nihilists, the internationals, the trades-unions, and the numberless other clubs, societies, or associations which periodically appear, particularly in times of depression, and which occasionally startle the world when the attempted assassinations of kings and emperors are or are supposed to be brought about by their influence. Germany is trying to repress them by severe laws, rigorously enforced without mercy to any one; but the disease, it seems to me, is merely put out of sight, not cured; the poison is driven inward, some day to break out in an unlooked-for place, and when least expected.

But it may be asked, what has all this to do with secondary technical education? For if Germany, with her well-established schools of the kind advocated in this paper, is more disturbed than any other country by the socialists, why should we wish to introduce them here? It must be remembered that in Germany the support of the army requires such a large part of the actual amount of wealth produced by each laborer, that hardly enough is left for the support of himself and his family, and that in many cases it is a desperate strug-

gle for the barest necessaries of life. When such is the state of affairs, it is not difficult to understand the dissatisfaction that exists among the poor there. The great point to be gained by the establishment of such schools, I would answer, is not only the building up around us of a set of foremen thoroughly trained, both theoretically and practically, familiar with the details of their business, with a strong *esprit de corps*, with a feeling of pride in the works where they have been educated, and with an interest in them that no salary or wages that they might be paid could alone give them. This is a great thing for the owners of the works and for their business success; but there is a point of much greater importance. Brains, though rare, are not distributed by any known rule among the people. A Stephenson may be born in the cottage of a common miner, and rise to teach the world the profession of engineering. A Franklin may force his way from a printer's case to the position of a great statesman and a great physicist. There are but few men of ability born each year; but they are as likely to be found in the humblest dwelling as in the palace.

One of the most important problems for a country is, how it can best utilize its most valuable raw material, so as to get the greatest possible effect from it. In the case of men of brains, the answer is very simple. Give them a chance to get an education by which their mental faculties can be afforded an opportunity of being used to advantage; make them capable of filling with credit to themselves the positions for which their natural abilities fit them. The same men that under the old system would have risen to the rank of master mechanic, will come to the front under the new; but they will come more quickly, and better prepared for the positions they may be called upon to fill. The greatest danger of nihilism, socialism, communism, call it as you may, comes from two classes,—first, the very ignorant, and secondly, from men of brains, who, finding themselves tied down so that there seems to be no outlet open to them from their present distasteful positions, strive to overthrow everything, in the hope that doing this they may be able to break away from the place to which they are bound, and to reach some more congenial position. If the boys about a mine, instead of spending their leisure time in taverns, or even worse places of resort, listening to the harangues of demagogues upon the wrongs of the working-men, should utilize it in such a school as I have been describing, they would be likely to feel that they have an interest in the prosperity of the country and in the stability of the government, and that there was a future open to them worth working

for. The older men, influenced more or less by the aspirations and opinions of their children, would be likely to sympathize with them. When passing through the coal or iron districts of our country, I have come upon groups of children whose appearance showed every evidence of neglect and want, and then Dickens's well-known lines, "There is not one of these poor creatures but sows a harvest that mankind must reap," have been brought forcibly to my mind. Common schools for the many, and technical education for those possessing intellectual ability enough to profit by it, although not sufficient alone to cure the evil, will do much toward it, and it is this view of the question of *secondary technical education* that I would ask the members of our Institute to bring to the attention of those to whose care the various industrial interests of the United States are intrusted; to explain to them the importance of such schools in training up foremen, in inducing good men who are anxious for the welfare of their children, to come to and remain at places where they can have such advantages. In converting the more intelligent of the young men about the works into efficient lieutenants in the management of the business, instead of allowing them to become dangerous, or at least troublesome agitators, a class of employés will be created who, while springing from the working-classes, familiar with their mode of thought, their aspirations and desires, will have had enough theoretical training to comprehend the value of scientific education, its methods and its aims, will be able to understand and talk with the engineer and manager, to explain to them better than the men themselves could what the employés' wishes or demands may be, and at the same time be more competent to bring home to the laborer the necessity of the changes which are proposed by the employer, or to show to the employer the reasons why he should yield to the demands of the men. The most difficult point in managing men is to find out when *we* are wrong and ought to yield.

The existence of such a class of men would do much to wipe out the jealousy which exists almost everywhere between the practical foreman and the educated engineer; for the former would be able thoroughly to appreciate the advantages which the higher education of the latter gives him in solving the different problems which so often arise in all engineering enterprises.

So impressed have we been by the great importance of this question, that it has been determined to establish a school of this kind at our collieries, the plan of which I shall sketch briefly. It is not intended, nor is it to be desired, that it should take the place of the ordinary

public schools, or that anything should be taught in it that can be learned in the latter. We have five good public schools at the works, and our object will be to induce parents to allow their children to remain there as long as they can, or, at least, do profit by the instruction given, and to discourage them from putting the boys to work picking slate too soon, for the sake of the small amount they can thus earn. If they could satisfy their consciences, or turn aside the reproaches of their neighbors by saying that they would send their sons to the apprentice-school at night, many a poor little fellow would be deprived of years of schooling, which he otherwise might get. The school is intended for those of the boys—the sons of our workmen—who have gone to work either with the miners, carpenters, blacksmiths, machinists, plumbers, masons, engineers, pumpmen, etc., about the breakers, or in the mines, with a view of learning their trade and beginning to earn their own livelihood. The course of study will certainly extend over two, and probably three years; this can only be determined by our success the second year. Any of the boys who have finished their studies at the public schools, or who are sufficiently instructed to begin with the others, and who express a desire to enter, will be admitted. There will be very little discipline of any kind, and every boy who strives to get on, even though he may not have great ability, will be encouraged and aided; those who show no inclination to work, or who are disorderly, will be dropped. The studies will be so arranged that each boy will always be able to work part time, and so gain practical experience, and wholly or partially support himself.

The absence of demand for coal, the insufficiency of the supply of cars, accidents at the mines and on the railroad, and other causes, often force us to suspend work for one or more days. These will always be utilized for study and practical work, such as drawing and mine surveying. We do not wish to do much night-work, although a reading-room and library will be opened every evening for the students who wish to avail themselves of it. The average boy cannot work all day and study all the evening. To properly arrange the time for study and for work, will be one of the most important and difficult of the problems with which we shall have to deal. It is important to give the boys all the instruction possible in the limited time and with the limited facilities at our disposal. But at the same time we must not expect too much of them or risk overtasking them. We firmly believe that "all work and no play will make Jack a dull boy."

The programme of the studies, as far as we have been able to arrange it, comprehends instruction in algebra, geometry, trigonometry, free-hand and mechanical drawing, with descriptive geometry, physics, chemistry, mineralogy and geology, mechanics, and the construction of machines, framing, mining, and mine surveying, English composition, bookkeeping, and writing. Our idea is to make the courses as practical as possible, and to devote particular attention to those subjects which are of special interest in our district. We shall have no course on metallurgy, for example. In mineralogy, we shall require the student to become familiar with the principal types of minerals only. In geology, much attention will be paid to the rocks and mineral deposits of Pennsylvania. We shall devote a comparatively large amount of time to the subject of framing, ordinary foundations, and the construction of such buildings as are required in our coal-fields. In machinery, particular attention will be paid to pumps, hoisting-engines, apparatus for preparing coal, steam-drills, etc. Mine surveying will be gone into with a good deal of detail, and in chemistry we shall try to impress upon the minds of the pupils those laws and phenomena which are of importance in understanding the ventilation of mines and the use of water in steam-boilers. In other words, we do not propose to make good chemists, or mineralogists, or geologists, but to endeavor to impart to the students as much knowledge, carefully selected from each branch of science, as will seem to us, upon mature reflection, to be most worthy of their attention during the limited number of hours that the boys can pass in the school.

Instruction will be given to the students every year by some competent physician upon the subject of the proper treatment, while the surgeon is being sent for, of those who may have been injured in or about the mines; particular attention being paid to those kinds of accidents, such as explosions, falls of slate, and burns, which are most likely to occur in such places.

The plan is far from being perfect, we know; but in a few days the first class will be organized and work will begin. As we progress, we shall undoubtedly have to modify much; but next year, when the second class is formed, we shall be better able to arrange its studies. It is undoubtedly an experiment, one that may not be thoroughly understood by those whom it is intended to benefit; but that it is well worth trying we are certain; and if we do not allow the first difficulties we meet to dishearten us, but push on, slowly

perhaps at first, but still without faltering or turning back, success will, we feel convinced, crown our efforts at last.

The establishment of such schools is of great importance to the country; and if those of us who are in a position to aid in the matter would give what assistance we can, and thus bring about the so-much-to-be-desired result, those who will fill our places when we are gone will, I am sure, have reason to thank us for having smoothed their path and removed many obstacles from it. We do not want an institution that a mechanic who has saved money and can pay the board and other expenses of the pupil, can afford to send his son to, but a simple school, open alike to the sons of spendthrifts, of drunkards, or of good mechanics, where a boy working every day to gain his livelihood can obtain enough knowledge to make him a better man and mechanic than he otherwise would have been. To do this, we need only, for a beginning, a couple of rooms, a few books, a little apparatus, a determination to succeed, the good-will and hearty co-operation of all about the works, and a resolution on the part of each true engineer to contribute his mite to the accomplishment of the great work.

At the conclusion of the address, Prof. Egleston and Mr. Holley spoke briefly of the importance of the subject presented by the President, and commended his great philanthropic zeal in establishing at his works technical schools for working boys.

The following papers were then read and discussed:

The United States Testing Machine at the Watertown Arsenal, by A. L. Holley, of New York City.

The Coal and Iron of the Hocking Valley, Ohio, by Dr. T. Sterry Hunt, of Montreal.

Prof. T. Egleston offered the following resolution, which was adopted:

Resolved, That a committee of three, of which the President shall be chairman, be appointed to consider and recommend a plan for the instruction of miners' children in the principles and methods of mining.

The President appointed Messrs. Egleston and Holley.

The second session was held on Wednesday morning at 10 o'clock. The President read a communication from President Gilman of the Johns Hopkins University, inviting the members to visit the lecture-rooms and laboratories of the University, and to meet the members of the faculty. The use of the library and reading-room was also offered to the members.

This session was devoted to the discussion of Dr. C. B. Dudley's paper, read at the Lake George meeting, on the Chemical Composition and Physical Properties of Steel Rails, which was commenced by the reading of a paper by Mr. Robert W. Hunt, of Troy, New York.

At the conclusion of this session the members visited the Johns Hopkins University, where they were courteously received in Hopkins Hall, by President Gilman, who gave a brief account of the founding of the University, and of the work which it was accomplishing. The members were then conducted through the buildings by the President and members of the faculty, to the different departments, and were afterwards hospitably entertained at luncheon.

The third session was held on Wednesday afternoon. The President appointed Messrs. Firmstone, Neilson, and Nichols, scrutineers to examine and report on the ballots received by the Secretary.

Mr. Charles A. Ashburner exhibited and explained, for Professor J. H. Harden, the improved tripod heads described in his paper on Imperfections in Surveying Instruments. The remainder of the session was taken up by the continuation of the discussion on Dr. Dudley's paper.

The third session was held on Wednesday evening. Mr. Charles Macdonald, of New York, exhibited two large steel bars, illustrating Mr. Kloman's process of rolling steel eye-bars, without upsetting, for bridge construction.

The following papers were then read and discussed:

The Pernot Furnace, by A. L. Holley, of New York City.

The Lake Superior Copper Rocks in Pennsylvania, by J. F. Blandy, of Philadelphia.

Indicator Cards from a Water-pressure Blowing-engine, by F. Firmstone, of Glendon Iron Works, Easton, Pennsylvania.

The fifth and concluding session was held on Thursday morning.

Mr. J. S. Alexander, Chairman of the Museum Committee of the Institute, read the following final report of the committee:

To the President and Members of the American Institute of Mining Engineers.

Gentlemen: Your Museum Committee beg to report that, since their appointment in October, 1876, frequent statements of progress have been made at regular meetings of the Institute, from which a general knowledge has been gained as to how they have discharged the duties assigned them. However, to make this, which is submit-

ted as a final report, more complete, a brief *résumé* of what has been done seems necessary.

Your committee, immediately upon the close of the Centennial Exhibition, adopted measures for packing and caring for the different donations, every care being taken to preserve the original labels, and, where desirable, diagrams were made of the arrangement and classification. A complete and carefully prepared list of the donations, with names of the donors, has been already published in Vol. V, Transactions of the Institute.

Your committee, at the same time, took steps to collect from the members of the Institute funds to defray expenses. A very cordial and prompt response was made, subscriptions coming in, not only from members residing near the proposed place of installation, but also from many in distant States, showing that the generosity of the donors of the collection, and the idea of preserving their gifts in the form of a Museum, were appreciated, and had awakened a widespread interest in the Institute. From outside the membership, encouragement was also received in the shape of liberal contributions to the fund from some of the large steel companies. The paid-in subscriptions amounted in all to $2149.77, which sum, as will be seen from the financial statement subjoined, was expended with the closest economy.

When seeking a location for the installation of the collection; your committee were generously offered, by the Pennsylvania Museum and School of Industrial Art, handsome accommodations in Memorial Hall, Fairmount Park, Philadelphia, for one year, without expense for janitorial and other care. The building before occupancy underwent complete renovation, and in due time the objects, comprising the numerous donations, were gathered into the rooms assigned them, from the different buildings in the Centennial grounds, and on July 1st, 1877, two large saloons, in which were arranged the large and more showy objects of the collection, were thrown open to the public. Of the appearance of the collection, and of the plan of its arrangement, the Institute had an opportunity of judging a year ago, when one session of the Philadelphia meeting was held in Memorial Hall.

The subscribed and collected funds became exhausted at this point, and your committee not deeming it advisable, in view of the uncertainty of the tenure of the space occupied being extended for a term of years sufficient to warrant further expenditure to either make

another appeal to the Institute or to incur any indebtedness, suspended installation.

Owing to the circumstances to which reference has just been made, one important feature of the installation plan of your committee was postponed, *i. e.*, the arranging of a cabinet collection illustrating the economic metallurgy of the world. Among the donations are full and valuable series of the ores of iron, copper, lead, zinc, tin, and antimony, together with fuels, fluxes, and the different metallic products representing the important metallurgical districts of nearly every metal-producing country on the globe, and furnishing material out of which to make a very complete classified collection, arranged by countries, or preferably by districts, showing the various metallurgical stages up to the finished product. It is hoped that means may be devised, by the society about to assume the charge of the collection, to utilize this valuable material in some such way.

As the year drew toward its close, your committee sought to perfect some arrangement by which a more extended and permanent installation would be secured without further expense to the Institute, but as nothing definite was reached immediately, the tenure of the space occupied was, by the courtesy of the Trustees of the Pennsylvania Museum and School of Industrial Art, extended. Pending these negotiations, the Smithsonian Institution, through Professor S. F. Baird, offered accommodations for the collection in the proposed National Museum in Washington; and the Metropolitan Museum of Art, in New York, recognizing its great value, made a very attractive offer to remove and install the collection in a handsome manner, at their own expense, in their new building in that city. The Pennsylvania Museum and School of Industrial Art, having also submitted a proposition providing for the permanent care of the collection and for the installation, free of any expense to the Institute, of the portion of the collection now stowed away, your committee, at a regularly called meeting held at 239 Broadway, New York, on January 23d, ultimo, resolved to accept the last proposition, and entered into the following written agreement to that effect.

Memorandum of Agreement, made this 23*d day of January,* 1879, *between the Pennsylvania Museum and School of Industrial Art, and the American Institute of Mining Engineers:*

It is proposed to allow the collections of the American Institute of Mining Engineers, now in Memorial Hall, Fairmount Park,

Philadelphia, to remain within the control of the Pennsylvania Museum and School of Industrial Art, perpetually, subject to the following conditions:

The Pennsylvania Museum and School of Industrial Art shall furnish from funds which they may receive from the State of Pennsylvania, the City of Philadelphia, or from any other source, the necessary show cases for the exhibition of such part of the collections as shall, in the opinion of the Museum, require such show cases.

The American Institute of Mining Engineers shall supervise the labelling and cataloguing of the collections, for which purpose the Institute will nominate five members annually to the Trustees of the Museum, from whom the Trustees shall elect one person to serve on the Committee of the Museum, having charge of said collections; and if the Institute shall fail to make such nomination, the Trustees may elect the member of the committee aforesaid, from the body of the Institute.

The name of the American Institute of Mining Engineers is to appear on labels and catalogues of the collections, and in a conspicuous place within the room or rooms containing the same.

Members of the American Institute of Mining Engineers shall be admitted to the building at the same times, and have the same privileges as to visiting the collections contained therein, as are enjoyed by members of the Pennsylvania Museum and School of Industrial Art, for which purpose the member's ticket of the American Institute of Mining Engineers, accompanied by a personal card, to be deposited at the door, shall be taken as if the member's ticket of the Pennsylvania Museum and School of Industrial Art was exhibted at the entrance.

The whole management, control, and exhibition of the collections shall be in the hands and under the control of the Pennsylvania Museum and School of Industrial Art.

The Pennsylvania Museum and School of Industrial Art will not insure the collections, and shall be in no way responsible for their safe keeping; but will give them the same care and protection as they do the objects and works of art of their own in the Museum building, and in no case shall the Museum be liable for any loss or damage to said collections, arising from any cause whatever.

It is understood that the American Institute of Mining Engineers shall be at all times ready to co-operate with the Pennsylvania Museum and School of Industrial Art, with a view to the increase of the collections and furthering their usefulness, and any and all ad-

ditions which the Institute shall acquire, or make, or cause to be made to said collections, including all collections acquired by said Institute, suitable to form part of said collections, shall, at the option of the trustees of the Museum, be deposited with and form a part of said collections in said building.

While at the Amenia meeting, in October, 1877, the Institute passed a resolution authorizing your committee to make such disposition of the collection as in their judgment would be advisable, they nevertheless, recommend that the Institute ratify their action, and pass the following resolution:

Resolved, That the President be authorized and requested to sign a contract embodying the provisions of the preliminary agreement, with such modifications and alterations as may be agreed upon between the Council of the Institute and the Trustees of the Museum, as necessary and desirable to carry out the spirit of the preliminary agreement on behalf of the American Institute of Mining Engineers, and that the Secretary be requested to forward the same to the President of the Pennsylvania Museum and School of Industrial Art for signature on behalf of that institution, and to request the return of one copy thereof, for filing with the records of the Institute.

At the same meeting your committee passed the following resolution:

Whereas, In consideration of the liberal offer of the Metropolitan Museum of Art, to exhibit the collection of the American Institute of Mining Engineers, a similar offer from the Pennsylvania Museum and School of Industrial Art, having been accepted by the Institute, therefore,

Resolved, That the Pennsylvania Museum and School of Industrial Art, be requested to tender to the Metropolitan Museum of Art any such duplicate specimens as may not make the collection exhibited by the Pennsylvania Museum and School of Industrial Art less complete and instructive.

Owing to the fact that the Institute is not an incorporated body, difficulties arose preventing your committee availing themselves of any existing statute to relieve certain parts of the collection from duty. The dutiable material was appraised, and upwards of $2000 would be required to pay the duties charged thereon. A resolution remitting these duties has been before Congress for upwards of one year, receiving the assiduous care of the Hon. Abram S. Hewitt, ex-President of the Institute; but under the transfer to the Pennsylvania Museum and School of Industrial Art that society may give a bond in its corporate capacity indemnifying the government against loss, should any of the dutiable material in the collection ever be sold

for commercial purposes, and thus comply with the statute governing such cases. A bond of this kind is now being prepared, and its execution will relieve the collection from customs *surveillance*. Acknowledgments are made to Mr. Hewitt for his efforts to secure the desired Congressional relief, and also to the customs officials, who have been very patient, always favoring the Institute to the utmost limit of their authority.

Before closing this report, reference should be made to the request mentioned in former reports as having been made by the German Government for a collection of Claiborne (Ala) shells, and Silurian fossils from the State of New York. Your committee have collected some specimens, but the desire has been to make the collection as presentable as possible, and they would again urge upon members to send in additional specimens; and, further, would recommend that this matter be placed in charge of a special committee.

ABSTRACT OF RECEIPTS AND EXPENDITURES OF THE COMMITTEE.

Receipts, ...		$2149 77
Expenditures:		
Packing and moving to Memorial Hall, . .	$607 90	
Office expenses, including printing and postage, .	267 59	
Salary of Superintendent of Installation and clerk,	284 88	
Installation, ...	941 65	
Incidentals, ...	11 80	
Fees of Custom-house broker, ...	21 20	
Balance on hand, ...	14 75	
	$2149 77	$2149 77

It should be added that a sum of one thousand dollars has been pledged by a number of Philadelphia members of the Institute and others interested in the successful installation of the collection under the arrangement made with the Pennsylvania Museum and School of Industrial Art.

Your committee thus return the trust committed to them two and one-half years ago, and request that they be discharged.

Respectfully submitted,

JOHN S. ALEXANDER,
Chairman.

February 18th, 1879.

The report of the Museum Committee was accepted, and the committee discharged.

The resolution embodied in the report was by vote, adopted.

Mr. Alexander mentioned that the Council of the Institute had suggested a few alterations in the original agreement as read, to

which it was supposed that the Pennsylvania Museum and School of Industrial Art would agree, and he, therefore, moved that the accomplishment of this agreement be referred to the Council of the Institute with power to act. The motion was carried.

Professor Egleston offered the following resolution, which was adopted:

Resolved, That the Council of the Institute appoint a Museum Committee of five members of the Institute, whose duty it shall be to visit the collection at least six weeks before each annual meeting, and report at this meeting to the Institute its condition, and whether the agreement entered into with the Pennsylvania Museum and School of Industrial Art, is being carried out.

Professor Egleston then offered the following resolution:

Resolved, That the thanks of the Institute be presented to Mr. J. S. Alexander, for his efficient services in collecting, arranging, and administering the Museum of the Institute.

Dr. Raymond moved to amend the resolution, by inserting after the name of the chairman of the committee, "and the other members of the Museum Committee." The resolution as amended was unanimously carried.

Professor Persifor Frazer, Jr., and Dr. T. Sterry Hunt, spoke in continuation of the discussion of Mr. Blandy's paper of the previous session.

The following papers were then read:

The Bradford Oil Region, McKean County, Pennsylvania, by Charles A. Ashburner, of Philadelphia.

The Ammonia-Soda Process, by Oswald J. Heinrich, of Drifton, Luzerne County, Pennsylvania.

The Great Blast at Glendon, by Ellis Clark, Jr., of Glendon Iron Works, Easton, Pennsylvania (read by Mr. Frank Firmstone).

An Improved Method of Cornish Pitwork, by Ellsworth Daggett, Jr., of Salt Lake City, Utah (read by Dr. R. W. Raymond).

The Furnace Assay of Platinum Alloys, by Nelson W. Perry, of Cincinnati (read by the Secretary).

The Determination of Silicon in Iron and Steel, by Dr. Thomas M. Drown, of Lafayette College, Easton, Pennsylvania.

An Improved Universal Suspended Hydraulic Lift, by J. A. Herrick, of Nashua, New Hampshire (read by the Secretary).

The following papers were then read by title:

The Sullivan, Maine, Silver Discoveries, by C. W. Kempton, Sullivan, Maine.

An Index of American Geological Reports, by Professor Frederick Prime, Jr., of Philadelphia.

Review of the Methods of Determining Phosphorus, by Professor F. C. Blake, of Lafayette College, Easton, Pennsylvania.

The Secretary then read the following report of the Council:

The Council of the Institute, in accordance with the rules, makes the following report to the Institute on the work of the past year.

Owing to the amendment to the rules at the meeting in May, 1878, changing the time of the annual meeting from May to February, there have been but two meetings held since the last report of the Council, viz., at Chattanooga, Tenn., in May, and at Lake George and Lake Champlain, in October, 1878. These meetings were memorable not only for their large attendance and professional interest, but mainly on account of visits made to places of mining and metallurgical importance, and for excursions to places of natural beauty. The members attending these meetings will long remember with pleasure the delightful trips on the Tennessee River and the grandeur and beauty of Horicon.

The accession to the membership has been 46 members and 13 associates. During the same period 26 have resigned, and 38 have been dropped from the lists owing to being in arrrears for dues for three years. Two members, Robert H. Gould and Walter Phelps, have died. The total membership of the Institute consists now of 5 honorary members, 52 foreign members, 543 members, and 134 associates, a total of 734.

The number of papers read at these meetings was twenty-eight, most of which have been printed and circulated. The system of pamphlet publication of the papers read before the Institute "*Subject to Revision*," in advance of their appearance in the volumes of Transactions, has now been fully tried, and as far as the Council has learned, seems to find favor with the majority of the members.

Vol. VI Transactions, covering the period from May, 1877, to February, 1878, has just been issued, and will be distributed to members immediately after this meeting.

The reports of the Secretary and Treasurer, duly audited, show receipts for the year as follows:

Secretary's and Treasurer's Statement of Receipts and Disbursements from May 3d, 1878, to February 13th, 1879.

DR.

Balance at last statement,	$80 83
Received for dues from members and associates,	4714 00
" from sale of Transactions,	170 00
" " " " volume on Technical Education,	1 50
" from authors for pamphlet copies of papers,	190 29
" for binding Transactions,	61 50
Interest,	8 77
	$5226 89

CR.

Paid balance due for printing Vol. V Transactions,	$401 87
" on account, for printing Vol. VI Transactions,	1200 00
" for printing pamphlets,	754 50
" for printing circulars,	97 55
" for engraving,	505 41
" for freight and expressage,	33 68
" for stationery,	24 25
" for telegrams,	23 33
" for fire-proof safe,	50 00
" for binding exchanges,	145 64
" for distributing Vol. V in Europe,	24 77
" for postage,	235 15
" for subscriptions to Engineering and Mining Journal (old account),	3 00
" for Secretary's salary, nine months,	1500 00
" for Secretary's expenses attending meetings,	102 97
Balance,	$124 77
	$5226 89

There remains unpaid on Vol. VI Transactions $627.91, leaving a net deficit of $503.14. This amount will be met, without doubt, by the receipts for the three months remaining of the year since the last annual meeting.

The Institute has recently received from the Russian Imperial Department of Mines a collection of twenty-eight Russian minerals in exchange for samples of coke and coal given to Prof. Nicolsky during the Centennial year.

The scrutineers appointed by the President reported the following officers elected:

PRESIDENT.

E. B. Coxe, ... Drifton, Pa.

VICE-PRESIDENTS.

H. M. Howe, ... Troy, N. Y.
R. H. Richards, ... Boston, Mass.
Samuel Thomas, ... Catasauqua, Pa.

MANAGERS.

J. A. Church, ... Columbus, O.
W. E. C. Coxe, ... Reading, Pa.
J. F. Lewis, ... Amenia, N. Y.

TREASURER.

Theodore D. Rand, ... Philadelphia, Pa.

SECRETARY.

Thomas M. Drown, ... Easton, Pa.

The names of the following persons, proposed for members and associates of the Institute, were presented, with the approval of the Council, and they were unanimously elected:

MEMBERS.

Alfred F. Brainerd, ... St. Albans, Vt.
George W. Bramwell, ... Drifton, Luzerne Co., Pa.
Henry Burden, ... Troy, N. Y.
Maurice Chaper, ... Paris, France.
John W. Cloud, ... Altoona, Pa.
Thomas Couch, ... Frisco, Beaver Co., Utah.
T. R. Countryman, ... Hastings, Minn.
George A. Crocker, ... New York City.
E. B. Dorsey, ... San Francisco, Cal.
Patrick Doyle, ... Perak, Straits Settlements, E. I.
Theo. N. Ely, ... Altoona, Pa.
Edward L. Ford, ... Springfield, Ill.
H. C. Frick, ... Pittsburgh, Pa.
Chester Griswold, ... New York City.
Henry C. Grittinger, ... Cornwall, Pa.
Prof. James Hall, ... Albany, N. Y.
Jed. Hotchkiss, ... Staunton, Va.
Eliot C Jewett, ... St Louis, Mo.
Isaac G. Johnson, ... New York City.
Ed. de Laveleye, ... Liége, Belgium.
George Russell Lincoln, ... Steel Works, Dauphin Co., Pa.
F. A. Lowe, ... Silver Islet, Ontario, Canada.
Maj. John R. McGinness, ... St. Louis, Mo.
Carlos W. McKinney, ... Scranton, Pa.
De Courcey May, ... Baltimore, Md.
George S. Morison, ... New York City.

George C. Munson, ...	Rosita, Colorado.
James W. O'Grady, ...	New York City.
Harry S. Peelor, ...	Johnstown, Pa.
David Shaw, ...	Pittsburgh, Pa.
Porter W. Shimer, ...	Alburtis, Lehigh Co., Pa.
Robert R Singer, ...	Pittsburgh, Pa.
M. V. Smith, ...	Philadelphia.
Sebastian Stutz, ...	Pittsburgh, Pa.
Edwin Thomas, ...	Alburtis, Lehigh Co., Pa.
W. W. Van Voorhis, ...	Manhattanville, N. Y.
Charles E Wait, ...	Rolla, Phelps Co., Mo.
John R. Williams, ...	Johnstown, Pa.
Jones Wister, ...	Harrisburg, Pa.
Theodore G. Wolf, ...	Scranton, Pa.
Fred. W. Wood, ...	Steel Works, Dauphin Co., Pa.

ASSOCIATES.

H. K. Bridgman, ...	Carondelet, Mo.
James Constable, Jr., ...	Constableville, N. Y.
Charles C. Dodge, ...	New York City.
Ezra B. Ely, Jr., ...	" " "
Walton Ferguson, ...	" " "
Henry D. Hibbard, ...	Providence, R. I.
Charles S. Hinchman, ...	Philadelphia.
Robert A. Shillingford, ...	"

The status of the following associates was changed to member:

A. W. Humphreys, ...	New York City.
H M. McIntire, ...	Menlo Park, N. J.
C. Henry Roney, ...	Philadelphia.
Nelson W. Perry, ...	Cincinnati, Ohio,
A. F. Schneider, ...	Salt Lake City, Utah.

The following resolution was passed unanimously:

Resolved, That the thanks of the Institute be presented to the President and Professors of the Johns Hopkins University, for their cordial reception, and to the Baltimore and Ohio Railroad and Pennsylvania Railroad companies for courtesies tendered.

The President then announced that the next meeting of the Institute would be held in Pittsburgh, and declared the meeting adjourned.

PAPERS

OF THE

BALTIMORE MEETING,

FEBRUARY, 1879.

THE PERNOT FURNACE.

BY ALEXANDER L. HOLLEY, C.E., LL.D., NEW YORK CITY.

THE Pernot system of rotating and withdrawing the hearth of a Siemens regenerative gas furnace for the production of Martin or open-hearth steel, is, perhaps, the most conspicuous of the several improvements which have lately been made in the steel manufacture. These several improvements in the open-hearth plant are not combined in any existing works, but most of them will be combined in not less than three works now erecting in the United States; and it seems probable that their joint result will not only reduce the cost of products of ordinary grades to Bessemer rates, but that it will produce new and better grades of products.

The open-hearth steel furnace and its operation are so generally understood that but a few words of general description are necessary. The ordinary furnace is not unlike the common reverberatory puddling furnace, except that an intense, uniform, and controllable temperature is maintained by the Siemens gas regenerative system.

The materials employed are various: melted or unmelted pig with ore, and with or without scrap; pig iron purified from silicon and phosphorus, with or without scrap; pig iron and scrap, melted together in a cupola, or charged hot or cold, together or separately, into the steel furnace; a pig-iron bath, and hot or cold steel or iron scrap, direct sponge or Catalan or puddled blooms charged into the bath.

The operation is, to a greater or less extent, according to the nature of the materials used, a desiliconization and decarburization of the pig or other crude iron, and its dilution by already purified materials. Manganese is employed at the end of the operation to remove oxide of iron and silica, a regulated amount of manganese remaining in the product.

Figs. 2 and 3, Plate IV, represent the hearth and roof of the Pernot furnace; Fig. 1 shows, in cross section, the general arrangement of open-hearth plant with Pernot furnaces, as generally designed by the author, and now building at two works. The engrav-

ings will be farther explained. The regenerator may be of any of the modern Siemens types.

The hearth has a plate-iron bottom, the sides being cast-iron staves secured by an upper ring, and easily replaced. The hearth stands on a cast-iron spider, which only touches the bottom plate at points, so that the spider does not get hot; the oil on the wheels is not burned. Indeed, one may touch these parts with the hand. No water is ever played upon any part of the apparatus to keep it cool. The spider rests upon four or six conical wheels, which roll on a circular railway cast upon the frame of a car. A small steam-engine, situated conveniently behind the furnace (see Fig. 1), drives a small gear which engages a large gear bolted in segments to the spider. The axis of the hearth is inclined five or six degrees from the vertical, and the speed of rotation is three or four revolutions per minute.* A roll train coupling on the driving shaft may be quickly disengaged when the hearth is to be drawn out. The car upon which the hearth revolves has four or six wheels, and may at any time be pulled out from under the furnace roof.

The joint between the revolving hearth and the stationary roof is simply a three-inch space filled with fire-sand, excepting a mere slit at the top. As there is in the furnace never more than a very slight plenum or a very sight vacuum (the former is desired and readily maintained); the sand is blown neither out nor in, and is a perfectly efficient packing. The bath is said never to have slopped over into the joint; if it did, no harm would be done; the bath in the Pernot puddler does rise and run into the joint, but without giving trouble.

The roofs and the sides of the hearth are made of silica or other suitable brick. The bottom of the hearth is made of fire-sand, rammed hard *while the hearth is out.* It is, hence, much more durable and more easily set than the stationary bottom. This is proved by the fact that but fifteen minutes are required to repair the bottom and tap-hole after a heat, while from one to two hours are occupied in similarly repairing stationary furnaces.

Each 8-ton furnace has four producers of the ordinary Siemens pattern and size.

The Pernot hearth is shown by Figs. 2 and 3, Plate IV, with its old form of roof on the left, and its new form on the right. In 1874 I had occasion to examine and report upon the very satisfactory out-

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* Experts do not get a satisfactory explanation of this slow rotation. Probably little effort has been made to ascertain the best speed.

put of the furnaces at St. Chamond,* in France. But the construction of the roof and ports presented some practical difficulties, which I found to be entirely remedied in 1877. No further changes have since been required, and the system is no longer in any sense experimental.

The old roof was flat, and, consequently, weak in itself; and it did not receive a continuous support from the circular iron framework which rests on the piers containing the vertical ports. The roof was necessarily high above the bath, and too flat to deflect the flames well upon the metal, so that there was a waste of heat.

The new roof is a flat dome, resting upon a continuous skew back, which is supported by the circular iron frame. This strong dome-roof may be pierced for ports and door, without being materially weakened. The ports lie on top of the roof, instead of underneath it, as in the old furnace. The new roof may also be lower than the old one—a most important feature—the flame is thrown straight down upon the bath.

The Pernot system has the following advantages over the fixed hearth:

1. *Mechanical agitation of the bath.*—Seeing that violent agitation, by promoting chemical reactions, completes the oxidation of silicon and carbon in the Bessemer cast-iron bath in fifteen or twenty minutes, while a similar oxidation of impurities in the open-hearth process, by means of ore without agitation, takes ten hours, it is obvious that the time of the operation is shortened in proportion to the amount of agitation. Shortening the time of course increases the output of a given plant, and decreases fuel, attendance, and expenses generally.

It has been objected that shortening the open-hearth operation is likely to concentrate phosphorus, as the Bessemer process does, but that a prolonged oxidizing action tends to remove it. This may be true, but in a very limited degree; the prolonged high temperature of the open-hearth bath contributes to the retention of phosphorus in the iron. The objection, however, becomes quite unimportant in view of the present systems (which will be further referred to) of dephosphorizing the materials before they are put into the open-hearth bath.

The mechanical agitation in the Pernot furnace is the constant rolling down hill of the fluid materials in the hearth and their con-

* Mr. Charles Pernot is the engineer of the "Société des Forges et Aciéries de la Marine," late Petin, Gaudet & Co.'s Works, at St. Chamond.

sequent turning over, stirring and fresh exposure to oxidizing influences. At the same time the solid materials are at each revolution raised into the full action of the flame, and then dipped under the bath, which coats them with slag, and prevents their oxidation.

2. *Heating the bottom sides* of the hearth is an advantage of rotation and of the inclined axis. The side that is lifted out of the bath at one-half revolution is exposed to the flame, and carries its heat under the bath at the next half revolution. Thus the chilling and sticking of the charge on the bottom may be largely prevented.

3. *Facility of charging.*—The simple throwing in at the door of the solid materials, which the revolving of the bottom will distribute, is an obvious advantage over distributing the materials all over a stationary hearth by means of a peel and with much more hand labor.

4. *Facility of repairs*, due to the removable hearth, is one of the most important advantages of the Pernot system. I had an opportunity to test the statement, current about the works, that the repairs could be made "over Sunday."

An 8-ton furnace, with new roof and ports, had run two months without any repairs. It had made, on soft plate and gun steel, 12 turns per week, and 1.66 heats per turn, yielding some 1300 tons in all. I saw a partially melted heat in the furnace late on Saturday afternoon; it was tapped about eleven o'clock, P.M, and the hearth was soon after pulled out. At ten o'clock, A.M., Sunday, I saw the roof and hearth repairs in process of completion; the hearth was run under a little after noon, and I saw a heat tapped out at half-past ten o'clock, Monday morning. About half the roof, after this two months' use, was very good. The part lying between the ports had been two-thirds burned through, and had to be nearly all renewed. The roof near the door had been a little cut away. A new ring of bricks was set around each port, and the covers of the ports (bricks in clamps), above their entrances to the furnace, were wholly renewed. With the old form of roof, the bottom of the port just over the hearth would not stand above 50 heats; with the new form, the whole skew-back supporting the dome-roof seemed perfectly good. The silica bricks used (they were not the best) were quite friable, and appeared to have broken away, rather than burned out, in some places.

The parts of the roof most exposed to heat in action are most exposed to cold air when the hearth is run out; and as the clamps forming the port covers are pulled off at the same time, the parts to

be cooled are only thin walls exposed on both sides to the air, and not receiving heat from the regenerators. I could, therefore, bear my hand on this roof in less than twelve hours after a soft steel charge had been tapped from under it. And the rapidity of repairs prevents the regenerators from cooling much, so that the furnace is quickly ready for another charge. Seeing that the repairs mentioned could be made, or that a whole roof, if necessary, could be renewed every Sunday, as well as every month or two, it is obvious that no important delays due to maintenance can occur with the Pernot system. The roof and ports over a stationary hearth, however, cannot be got at for repairs until the hearth and the regenerators are cooled, and this must obviously take four to six days, unless water is used; and water tends to injure the brickwork.* Pipes from a fan are now arranged to blow air down upon those parts of the roof which are most heated internally, with good results. The greater part of the roof stands 250 to 300 heats.

The repairs of the Pernot *hearth* are also facilitated by its removal. As it is thicker than the roof, it does not cool as quickly; water is, therefore, used, but it only touches the brick to be renewed, and does not injure the brickwork of the furnace proper. The 8-ton furnace hearth is lined with 4½-inch brick next the sides, and two rows of silica blocks, 9 inches thick each. The bottom is 8 to 9 inches of pure silica sand, mixed with a little sandy fire-clay, which slightly fluxes it, so that it will set hard. After the above two months' running, the bottom was good—it had been renewed by throwing in sand where needed from time to time; but the whole of the first course of 9-inch side blocks, and about half the second course, had been cut away. These sides are too steep to be repaired by sand when the furnace is running; and the mechanical action of the bath wears them. The blocks must, therefore, be quite hard and refractory. Seeing that Bessemer vessel linings stand the same chemical and a much more violent mechanical action for a much longer time, and that there are the best possible facilities for ramming a ganister lining into a Pernot hearth, it would seem that a much better practice than that at St. Chamond is possible. The bottom can, however, be repaired every Sunday, if necessary; and duplicate bottoms, like those of Bessemer vessels, may, of course, be used.

5. *The safety and convenience* of the bottom are increased in various

* In the stationary 6-ton and 8-ton furnaces, twenty-four hours must elapse before brick can be laid; then twenty-four to forty-eight hours more to get the furnace ready for gas, and finally twenty-four to forty-eight more to get out the first heat. The time varies with the extent of repairs.

ways by the Pernot system. In case the steel gradually works through the bottom, it can be detected at once by the heating of the bottom plate, and checked by a stream of water. This is also the case, in a less degree, with the latest stationary hearths, which stand over a vault. But if the break is on either side of the Pernot bottom, that part can be turned up so as to have but 2 or 3 inches of metal over it, when it can be readily chilled.

If the metal cannot all be tapped out, or if a "bear" occurs from any cause, the scrap, and the lining also, if necessary, can be hoisted straight out with a crane, while the whole roof, if not the side walls, must be removed to get a "bear" out of a stationary hearth. The convenience of frequently repairing the hearth prevents the accumulation of scrap in it, and also the probability of the charge breaking through. The latter disaster has never occurred at St. Chamond. The little scrap that stays in the hearth when the charges are cold is melted out in the usual way.

The operation in the Pernot furnace may be the same in all respects as that in the stationary hearth, except that it is more rapid. Pig and scrap of various kinds, and in various proportions, are used at St. Chamond. The pig and ore process has not been employed,* but there is obviously no reason why it may not be. There is a space of some 6 inches for slag between the top of the bath and the lowest part of the joint, and more may be left, if necessary. The rotating hearth offers the best possible facilities for tapping off slag. By stopping the rotation, opening a tap-hole at suitable height, and then rotating the hearth till the steel just reaches the bottom of this hole, all the slag may be forced out by means of a suitable tool worked through the door. Skimming the slag from soft charges is regularly practiced at Creusôt.

The sizes, working and output of the Pernot furnaces at St. Chamond have been somewhat changed from time to time. In 1874 there were two "five-ton" furnaces, which I saw making five 4-ton heats in twenty-four hours. The regenerators were afterward slightly enlarged, and the hearth was lined thinner, so as to hold seven to eight tons. Late in 1876 the new form of roof was introduced, and a few months later I saw an improved practice. The larger part of the material was charged cold, and the average number of 7-ton heats was three and one-third per twenty-four hours. The regener-

* The pig and ore process is not employed at Terrenoire either, nor at Creusôt. At the latter place a little ore is sometimes thrown into a pig scrap bath to heat it.

ators were then working a little cold, on account of bad drainage around them. Now the material is all charged hot, the regenerators work well, and the average number of heats is four per twenty-four hours, or twenty-four charges per week, on ordinary steel. On very soft steel the charges are three or four less per week. In the former case the bath is not fully decarburized, and this saves time. When making soft steel, the bath is fully decarburized, and then brought up to the exact point by ferromanganese.

The actual time of making a heat is about five hours. One hour is allowed to clear and repair the bottom, and to charge the furnace by the awkward means used.

Table I gives some particulars of the practice in 1876–77, and Table II gives some particulars of the later practice, and also of costs.

TABLE I.

"Eight-ton" Pernot Furnace Practice in 1876–77.

Total number of turns in four weeks,	44.
Average number of operations per twenty-four hours,	3.27
" tons of ingots per twenty-four hours,	21.73
" " " per operation,	6.64
Producer coal per ton of ingots, pounds,	645
Preheating furnace coal per ton of ingots, pounds,	118
Steam coal, estimated, pounds,	100
Total coal per ton of ingots, pounds,	863
Average total charge per ton of ingots, pounds,	2,373
" loss on raw material to ingots,	5.94 per ct.

TABLE II.

"Eight-ton" Pernot Furnace Practice in 1878.

One furnace, working twenty-two days and two hours, making chiefly mild steel for cannon.

Labor (working two or three furnaces).

	Wages of each man per ton.	
One head gasman,	$0 09	$0 09
Two helpers,	4½	9
Two coal wheelers,	4½	9
One man runner,	9	9
One helper,	7	7
Two sweepers,	4½	9
Two melters,	9	18
One fireman (preheating furnace),	6	6
Two helpers,	4½	9
"	6	12
Three pitmen,	6	18
Four stockers,	4½	18
Total,		$1 33

Labor paid by the day (all on day turn only, except two engineers) per ton as follows,	$0 32
Total labor,	1 65

Average daily production (twenty-four hours) in May = 24 79 tons, which makes day labor per ton—$7.95 ÷ 24.79 = $0.32.

	Wages per day.	
Two engineers at pump,	$0 90	$1 80
One general man at pump,	65	65
Two engineers at furnace,	70	1 40
One laborer,	65	65
" clayman,	70	70
" roaster,	50	50
" sweeper,	60	60
" chipper,	90	90
" helper,	75	75
Total,		$7 95

Consumption of materials—iron, scrap, and spiegel, pounds,	1,280,372
Product, pounds,	1,224,002
Loss,	56 370 or 4.3 per ct.
Average weight of charge, tons,	6.1
Number of charges per twenty-four hours,	4.09
Production, tons,	24 79

The labor at St. Chamond is excessive, and may, of course, be reduced by better handling arrangements. The coal for the steel furnace has been as low as 600 pounds per ton of ingots.

The preheating furnaces, which two years ago were used to heat spiegel and some scrap (two tons at the most for one steel furnace), have been enlarged to preheat all the pig and scrap. Formerly, the pig and nearly all the scrap were charged together cold; now they are charged together hot, and hot scrap is added as required.

When it is time to cast, the hearth is turned so as to bring the tapping spout under the deepest part of the bath. Should any accident occur to the breast or to the ladle, the spout may be turned back to stop the flow of metal. After the steel has run out, the spout may be turned a little so as to run the slag into a car. An extra breast should be provided to prevent too much wear of lining around one breast.

The principal dimensions of the various Pernot furnaces at St. Chamond are given in Table III:

TABLE III.

Eight-ton Furnace.

	Ft.	In.
Diameter of shell of pan,	10	8
" inside of lining,	6	10
Depth of pan,	1	6
" bath centre,	0	8
Size of valves,	2	0
" one regenerator, 6 ft. 2 in. x 3 ft. 9 in. x	8	6
Total cubic contents of regenerators,	790 c. ft.	

Twenty-ton Furnace.

	Ft.	In.
Diameter of shell of pan,	17	2
" inside of lining,	13	9
Depth of pan,	0	16½
" bath in centre,	0	9
Size of valves,	2	9
" one regenerator, 10 ft. 2 in. x 3 ft. 8 in. x	8	6
Total cubic contents of regenerators,	1280 c. f.	

Puddling Furnace.

	Ft.	In.
Diameter of pan,	8	8
Depth of pan,	0	14
Size of fire-box, 3 ft. 3 in. x	4	10

The Twenty-ton Furnace.—The general dimensions of this furnace are given above. It has been run only at the rate of two heats per twenty-four hours. It is stated that it might be run faster, but that it makes all the steel at present wanted, most of its product being large castings for cannon, armor plates, and shafting. An eighty-ton hammer is nearly completed, and foundations are going on for three more twenty-ton furnaces, so that the size of furnace must be satisfactory. We know that twelve and fourteen-ton stationary furnaces make at least as many heats as five and six-ton stationary furnaces, and with a high economy of fuel. It is, therefore, impossible to believe that the large revolving hearth will not work as fast as the small one.

It should not, however, be surprising if this particular furnace *could not* make but two heats a day; its regenerator is but little over one-fourth the size of that of the Terrenoire twenty-ton furnace, and less than half the proportional size of those of standard furnaces. (See Table IV.) The arrangements for casting large ingots are at present so crude at St. Chamond that it is difficult to see how more than one charge per turn could be disposed of.

TABLE IV.

Comparing the General Dimensions of Various Open-hearth Furnaces.

FURNACE.	Horizontal area of bed—Sq. feet.	Vol. of gas regenerator. Cubic feet	Vol. of air regenerator. Cubic feet.	Total area gas ports Sq. in.	Total area air ports Sq. in.	Area of gas valve. Sq. in.	Area of air valve. Sq. in.	Vol. of (gas + air) regenerator, divided by horizontal area of bed.
Pernot .... 20 tons.	149 6	210 0	246.0	255	255	201.0	254 5	3 05
Otis Iron and Steel Works (1877)	115.5	201 1	290.8	486	648	346 4	346 4	4.26
" " " (1875)	92 6	210.4	309.4	396	495	346 4	346.4	5.61
Le Creusôt* .... 10 tons.	60.0	149.3	188.8	288	288	283 5	283.5	5 63
Landore .... 7 "	85 8	208.1	299 1	280	476	254.5	346.4	5.91
Landore .... 12 "	117 0	315 2	443.2	432	666	346.4	452.4	6.48
Panteg .... 14 "	135.0	384.3	548.2	....	....	346.4	346.4	6.90
New Siemens .... 10 "	128.2	429 9	617.9	315	665	452.4	572.5	8.17
Terrenoire .... 20 "	107.3	641.9	641.9	459	496	441.3	441 3	11.96
Crucible Steel	18.0	91.6	127.5	108	432	176 7	176.7	12.17

* The bed of this furnace was afterward enlarged to fifteen tons capacity. with regenerators unaltered.

Capacity.—The maximum capacity of the two 8-ton furnaces and the 20-ton furnace is about 30,000 tons per year. About 40 per cent. of the steel product goes into other uses than rails. The Bessemer plant at St. Chamond has been abandoned.

Cost of Furnace.—The cost of an 8-ton Pernot furnace and driving gear at St. Chamond is said to be \$7800, exclusive of producers, gas-flues, and stack. Of this, the entire cost of apparatus for rotating and withdrawing the hearth (excluding engine) is \$1200. The hearth and carriage of the 20-ton furnace weigh about 38 tons. The cost of the whole apparatus above ground, including engine, is stated to be \$9000; of the regenerators, \$1600 = \$10,600.

At St. Chamond, a 20-ton and two 8-ton furnaces are running, and three 20-ton furnaces are buildiug. There are elsewhere in France, two Pernot steel furnaces, also two in Austria and two in Russia.

The Pernot Puddler.—A few facts incidentally, about this apparatus and practice may be of interest. A large number of these fur-

naces are used abroad; the dimensions of the furnaces at St. Chamond are given above.

The best work is as follows: Number of charges per twenty-four hours with gray pig, nine; with white pig, twelve; weight of charges, one ton. Fuel, per ton of product, with gray pig, one ton; with white pig, 1650 to 1700 pounds. When on gray pig, the furnace stops about two hours, three times a week, to be fettled; on white pig it is fettled on Sundays only. Each furnace requires two puddlers, two helpers, and one fireman. One man attends to all the rotating engines. The labor of stirring the charge is very slight—merely holding a rabble in the door; the labor of balling and lifting out the metal is nearly as great as in hand puddling. The consumption of fettling is various, according to quality; the purification of product may be made pretty complete, but probably not so complete as by the Danks-Bouvard furnace, at Creusôt.

General Arrangement of Open-Hearth Plant.—Fig. 1, Plate IV, is a cross-section of one of the plants now erecting in this country. There are two 15-ton furnaces, having a common ladle-crane and two ingot cranes. The furnaces are set high, in order to allow a shallow pit, and a long furnace spout with a good fall, when required; also to bring the flues and the valves and the entrances to the regenerator chambers, near the general level. The stock must be raised by power to the charging door, even if the hearth of the furnace runs out on the general level. The advantages of elevated vessels and shallow pits—a short lift of *hot* material—have been abundantly proved in the Bessemer manufacture.

That part of the charging floor (12 × 16 feet in plan) immediately behind the removable furnace hearth, is a carriage, standing on the railway upon which the hearth also runs out. When furnace repairs are to be made, the section of floor is run back, and then the hearth is run out from under the roof.

Close behind the charging door of each furnace there is a hydraulic lift, so that either fluid materials may be run in a ladle on a car, from the blast furnace or cupola, or from any fluid-refining apparatus, and tipped directly into the steel furnace, without other transference or delay; or solid materials, cold or hot, may be brought on a car, either from the stock yard or from a heating or puddling or direct-process furnace, and pushed into the steel furnace door; the revolution of the hearth distributes the solid materials. The greatest convenience and celerity of charging is thus attained. There are

cupolas and refining apparatus just outside the open-hearth building, and the cupola lift also communicates with the open-hearth charging floor.

It seems appropriate in this connection, and, in view of the other improvements in open-hearth plant and practice which have been alluded to, that I should conclude with a brief review of the status and prospects of this manufacture in the United States.

While the Bessemer process has reached a high degree of perfection, so high, indeed, that the directions of great improvement are not obvious, the open hearth is availing itself of new accessories in every direction. The Bessemer looks back to a splendid development; the open hearth looks forward to a splendid development.

In this country, to-day, the Bessemer is by far the cheaper method of producing the ordinary and some of the finer grades of structural steel. The product is not only cheap, but when made of the best materials and with the skill and care that many of our experts can bestow, it is trustworthy and excellent. The open-hearth process, on the contrary, is performed mostly in small furnaces and in works too limited to fully economize labor and maintenance. It receives no aid from any preliminary preparing or refining operation. Excepting in a few details and in the arrangement of a few works, the plant is little better than it was five or six years ago. The practice, however, in most of our works, is excellent, and the product of some of them is not exceeded in quality anywhere in the world. For very soft, very hard, and very pure products, and for steel castings, the present open hearth has advantages over the Bessemer. The materials, however, are of the most expensive character.

But what are the indications for the future? The Bessemer must be chiefly confined to irons very regular in silicon, and hence the most costly to make; to irons pretty low in phosphorus* and so requiring ores that average the dearest; to crude irons that have not been purified from phosphorus, because they would then also be purified from silicon, and would not be hot enough to blow. If remelting the pig is saved by working directly from the blast-furnace, then the ores and the blast-furnace practice must be still more regular and costly.

The open hearth has the following advantages, some of them secured and some within reach:

* The results of the lime lining and lime flux, now on trial, may modify this conclusion.

1. It is not dependent for temperature on the ingredients of the materials: hence, so far as heat is concerned, it can digest anything that bears the name of iron.

2. It can, as well as the Bessemer, take its material direct from the blast furnace or from the cupola.

3. The greater part or the whole of its materials can be purified by a preliminary process, and hence the cheapest ores can be used, whatever their quality. This can be done in which ever of three ways is best suited to the particular case:

(*a.*) The direct process, as by the Siemens rotator. This is likely to prove the best of the purifying processes.

(*b.*) Fluid refining, as by I. Lowthian Bell's method. This is already practiced successfully on the Continent.

(*c.*) Mechanical puddling. This is best done at Creusôt. One Danks-Bouvard furnace produces twenty tons of blooms in twenty-four hours, from metal remelted and partially desiliconized in a reverberatory gas furnace.

4. Very impure scrap, such as old iron rails, may be purified, in connection with pig, by fluid refining.

5. These preparatory processes shorten the open-hearth operation, and so decrease the cost of production, and increase the yield of a given plant.

6. The large furnace also decreases cost and increases yield. The Terrenoire twenty-ton furnace works just as fast as the smaller ones, when each is worked up to capacity.

7. The Pernot revolving hearth already nearly doubles the output of a given regenerative capacity, and it greatly facilitates repairs.

All these improvements, most of which are well developed, are before the American open-hearth practice, and within its reach. Three establishments in this country are erecting large Pernot furnaces, with refining apparatus, and I shall be much surprised if a pair of these furnaces does not turn out above one hundred tons of ingots per day, at Bessemer rates. Other establishments are erecting open-hearth plants, essentially old-fashioned and cheap. The Bessemer experience has certainly proved that the better the plant, no matter what it costs, the cheaper the product.

The open-hearth process can obviously be carried on in connection with the Bessemer process to *the greatest possible advantage;* it can cheaply utilize, not only scrap of every kind, but pig iron unsuitable for Bessemerizing, which is always accumulating; and it can most

cheaply employ the rolling and finishing machinery, and the organization already existing.

The conclusion is inevitable that the open-hearth process is about entering upon a development which will largely increase the use of steel, not only as a substitute for wrought iron and cast iron, but with new adaptations and in new directions.

Discussion.

Mr. C. E. Stafford.—The open-hearth process is, as yet, in its infancy. It will, no doubt, in the near future, play a more important part in the production of steels of all grades; and is destined, as pointed out by Mr. Holley, to be at least a powerful rival, if not the successor of other processes at present in use.

While acknowledging the many merits of the revolving hearth, I think the fixed hearth will compare very favorably with it both as to cost of repairs and fuel economy.

At the Pennsylvania Steel Works, we have two open-hearth furnaces, of which the hearths were changed from six to fourteen (gross) tons capacity by bringing out the sides of the furnace; the hearth is now nearly octagonal in shape. This change was made without, in any way, altering the regenerator-chambers or the area of the ports. The two furnaces were placed so near together as not to allow room for the ports to be properly constructed.

The interval between repairs is seven to eight weeks. During the first three to four weeks of this period, the charge of 32,000 pounds of pig and scrap—charged cold and at one time—is in the furnace from eight to nine hours. After the fourth week the "time in furnace" gradually increases, because of the gas-chambers being too small. These chambers at the beginning of the campaign seem to have just enough heating surface and area for the free passage of the gas, with no margin for the decrease of effectiveness caused by the filling-up of the interstices of the regenerator bricks by the so-called "red soot," consisting of oxide of iron, fine sand, etc.

With these furnaces, during the first half of the campaign, or when in their normal condition of a well-proportioned furnace, the coal (Clearfield) consumed per gross tons of ingots produced is six hundred pounds.

The silica brick roofs and in-walls have been in service more than a year without repair. The usual repairs consist in the taking out of the regenerator bricks, and relaying them, and the facing of the ports.

With well-arranged gas and air ports, large regenerator-chambers and capacious slag-pockets, or, in a word, with a well-proportioned and properly constructed furnace, using silica bricks, or cooled alumina bricks, I see no reason why repairs should be necessary oftener than once in six or eight months.

When the furnace is in its normal condition, the time necessary for repairing the hearth or "making bottom" is very short, although we use fourteen to twenty hundred weight of ore to oxidize the carbon in the charges. The interval between the tapping of one heat and the beginning of the charging of the next is forty-five to seventy-five minutes.

I have given the above details of these furnaces, together with their working, to show that passably good results having been obtained even under disadvantages: much better results—both in regard to economy of fuel and of outlay for repairs—could reasonably be expected of a well-proportioned and properly constructed furnace of the same general type.

The ordinary and Pernot furnaces differ very much in the placing of the refractory materials forming the hearth; in the former there is a large mass of sand below the slag level which acts as a reservoir of heat, preventing that sticking of the metal to the bottom which is prevented in the latter furnace, with very thin bottom, by its periodical exposure to the direct heat of the flame. Even in this case, it seems to me, if the additional weight and space occupied would not be objectionable, a thicker bottom would be an advantage, especially if much cold stock is used.

The best construction of the ordinary or fixed hearth is to arrange it so that from a little below the slag level, the mass of refractory material should be as great as possible for the reason pointed out; above this level the layer should be comparatively thin and cooled by circulation of air, or other means to prevent excessive scorification of the sand by the very basic slag covering the metal.

In reply to an inquiry, Mr. Stafford gave the following dimensions of his furnace: Volume of air regenerator, 285 cubic feet; volume of gas regenerator, 183 cubic feet—together 468 cubic feet. The size of the hearth may be taken as twelve square feet, hence:

$$\frac{\text{Volume of air and gas chamber}}{\text{Area of hearth}}=\frac{468}{144}=3.2$$

THE UNITED STATES TESTING MACHINE AT WATEI TOWN ARSENAL.

BY ALEXANDER L. HOLLEY, C.E., LL.D., NEW YORK CITY.

THE 400-ton testing machine, ordered in June, 1875, by the Unite States Board appointed to test "iron, steel, and other metals," h: lately been completed at the Watertown Arsenal, thoroughly prove and accepted by the Board. The excellence of the machine in ever respect is more than satisfactory, and its accuracy is at first sigh astonishing, although an investigation of its principles must sho that if the weighing apparatus will weigh at all, it must do so wit perfect accuracy, because all its movements are absolutely withou friction.

The proof experiments were numerous, and the effects of reco after sudden ruptures at maximum loads, were watched with gre: care, but without much anxiety, because the weighing parts affecte are by no means delicate in structure, and their motion is almos infinitely small. Among the tests were the following:

A forged link of hard, wrought iron, 5 inches in diameter betwee the eyes, was slowly strained in tension, and broke short off with loud report at 722,800 pounds. The diameter before breaking a the point of fracture was 5.04 inches; after breaking, 4.98 inches.

In order to see if the weighing parts had been disturbed by th recoil, which was obviously near the greatest recoil the machine wil ever suffer, a horse-hair was next tested; it was 7-1000ths of an inc in diameter; it stretched 30 per cent., and broke at 1 pound. Othe horse-hairs vary in tenacity between 1 and 2 pounds. Of course, th accuracy of the machine on such delicate specimens, and indeed o specimens having some hundreds of pounds tenacity, has bee checked and proved by other weighing machines.

A 5-inch round bar, turned down to $3\frac{5}{8}$ inches diameter along th centre, was pulled apart at 430,200 pounds tension. Then some mor horse-hairs were tested; also copper wires $19\frac{1}{2}$-1000ths of an inch i diameter, which averaged 25 pounds tenacity.

Specimens were subjected to 1,000,000 pounds compression, although the contract calls for but 800,000 pounds. After these proofs delicate structures, such as eggs and nuts, were tested in compression, and violin strings in tension. It is unnecessary to multiply instances. It seems safe to conclude that bars and structures up to 400 tons can now be tested with perfect accuracy, and that there is no reason to fear the deterioration of the weighing apparatus.

It is not the purpose of this paper to describe the machine in detail, because foreign patents to the inventor and builder, Mr. A. H. Emery, are not fully secured. Speaking generally, the machine consists of a double-acting straining cylinder and ram on a carriage at one end, and a movable weighing apparatus at the other end. The two are connected by a pair of 8-inch screws 48 feet long. Nuts driven by shafting move the straining cylinder to different places on the screws, so as to test long or short specimens. The weighing apparatus has already been described in print as a reversed hydrostatic press, having diaphragms instead of pistons. The load is transferred, by means of a fluid (alcohol and glycerin), by a series of large diaphragms to a series of small ones, and finally, to a system of scale-beams. Thus a weight of 800,000 pounds, acting through an inconceivably small space, finally moves a finely graduated indicator at the rate of 1-100th of an inch per pound. It is allowed to move through a space of 2 inches, and is kept balanced by weights mechanically placed quickly on or off the scale-beam. One pound, in moving the indicator 1-100th of an inch, moves the platform against which the load presses 1-42,000,000th of an inch. The whole arrangement of the scale-beams, the adding and removing of weights, and the fast or slow, but always steady application of pressure, are ingenious and convenient in the highest degree. By means of universal joints, the pressure pipes are always connected to the straining cylinder, etc., whatever their positions. The steam-pump and the accumulator have cylinders and weights, respectively for high and low pressures, and the machine receives pressure without pulsation, from the accumulator, only when testing.

The machine was built at the works of the Ames Manufacturing Company, at Chicopee, Mass. The principal castings (80,000 lbs. of gun iron) were made at the South Boston Iron Works, and the steel and iron forgings at the Nashua Iron and Steel Works. The finished metal in the machine weighs 175,000 pounds, and includes pieces of 14,000 pounds down to those of which 250,000 would weigh 1 lb. The hydrostatic weighing platform of the machine was tested

to 1,500,000 pounds, but so perfectly frictionless is it that a horse-hair, under a breaking strain of 1 lb., had to move 24,000 pounds of metal. The workmanship is also remarkable. The 8-inch screws, 48 feet long, were fitted to gauges within one-thousandth of an inch in diameter throughout their length, and similar accuracy was maintained in other parts.

The cost to the government of the machine and appurtenances, was as follows:

The machine, with pump and accumulator,	$31,500 00
Erection,	4,000 00
Foundations and accumulator pit,	4,083 77
Travelling crane,	2,981 23
Steam-pipes for heating building,	439 52
Total,	$43,004 52

The Board had been convinced of the accuracy and the durability of the Emery weighing apparatus up to a few tons stress, but they were unwilling to risk the failure of so expensive a testing machine on this apparatus alone. So they added an independent weighing apparatus on the next best of the several plans submitted. This is the plan of Mr. Charles E. Emery—an excellent system, and vastly more accurate than any previously used, although much less sensitive than that of Mr. Albert H. Emery, the builder of the machine. It had long been suspected that the pressure of the fluid in the straining cylinder of a testing machine is sometimes very much higher than the pressure on the specimen, by reason of the friction of the piston packing, especially under great stresses. Mr. Charles Emery demonstrated to the Board that this packing friction could be so far overcome by revolving the piston by power, that it would move freely longitudinally, and that the fluid pressure in the cylinder would pretty accurately represent that on the specimen. A supplementary cylinder, on a carriage, was therefore placed between the straining cylinder and the specimen, and its piston was arranged to be revolved by the shafting before mentioned. The pressure per square inch in this cylinder would very nearly represent that per square inch on the specimen. But it was not an easy matter to construct a gauge which should perfectly measure even 3700 lbs. cylinder pressure per square inch. This, Mr. Albert Emery, however, accomplished on his reversed hydrostatic press system. Within the lower ranges of total pressure, these two weighing machines indicated so nearly alike as to prove that revolving the piston would

show approximate accuracy of pressure, but at the higher ranges, so great was the packing friction that the heavy machinery provided would not revolve the ram. It now seems probable that this supplementary apparatus will not be regularly used, although it may readily be made heavier, and it will always be valuable to correct the readings of the other apparatus. It is certainly worth many times its cost in proving the worthlessness of hydraulic testing machines as heretofore constructed. The readings of the permanent weighing apparatus, as compared with those of the cylinder gauge when the piston was not revolving, showed in some cases an error of 40 per cent.

I regret that I cannot now refer to other extremely valuable features of this machine, on account of Mr. Emery's patents. The importance of a testing machine of great power cannot be overestimated. Constructors are beginning to find out that they have been led astray by predicating the physical qualities of large bars on those of smaller ones. One might almost as well exhibit a brick as the measure of the strength of a wall. The very first high stresses put upon this machine were a striking commentary on the error referred to. The link which broke at above 700,000 pounds was sent out by the makers as "60,000-pound iron," but it broke at a little over 36,000 pounds. The bar which broke at above 430,000 pounds was made of the very iron which, having endured above 50,000 pounds per square inch in a 1-inch bar, broke at about 37,000 pounds per square in a 5-inch bar turned down to $3\frac{5}{8}$-inch.

But measuring the strength of large bars is not the only advantage of a large machine; it is equally important to determine the weaknesses of structures, and so to lead to the development of perfect forms. Given the strength of the individual pieces, it is impossible, for instance, to calculate the strength of a latticed column. But a testing machine that will take in a whole bridge post, or a whole section of top chord, and subject it to a regularly increasing and measured stress up to the point of destruction—such a machine develops structural defects, as well as the physical qualities of materials.

Comparative experiments on similar specimens, to test the accuracy of other machines, have not yet been made. The fluid pressure in the straining cylinder and the knife-edge weighing machine, or ordinary scale, are the only other systems. However they may answer for small stresses, it is probable that they are, as heretofore constructed, totally inadequate and misleading for great stresses.

The United States testing machine can apply 1,000,000 pounds compressive stress to specimens of any length up to 30 feet. It can apply 800,000 pounds tensile stress to links or specimens made so as to be held by pins of any length up to 37 feet. By a small addition to the machine, specimens not occupying more room than the straining link of the machine, can be tested up to about 45 feet length. The apparatus for transverse strains has not yet been applied, nor has the board had the means to supply many needed tools and instruments of precision for measuring the stretch of the specimen. Such, briefly, is the United States testing machine; an engine of power and precision, in which lie the possibilities of a revolution in the manufacture of iron, steel, and bronze, and in the proportioning and adaptation of structures. I use the word possibilities advisedly; the immediate probabilities of such a grand work are not conspicuous, for the Congress of the United States has refused to furnish the money to make the machine available. It has refused to continue the Board, and on the 30th of June next, according to law, the Board will die. It has even removed the machine from the custody of the Board to that of the Secretary of War. But it has done another thing; it has announced its own magnificent scheme for solving the problem upon which, more than on any other, the immediate improvement of structures depends,—the improvement of bridges, and ships, and iron buildings, and ordnance, and every kind of machinery. The scheme of Congress amounts to this,—anybody can send his materials to the Watertown Arsenal, and have them tested at cost, if there is anybody there to test them. Let us see how it will work.

There is a general call now for steel long-span bridges. Nobody knows, except approximately, the grade of steel required for the various kinds of stress, or the physical quality of bars of working sizes. Our knowledge of the strength of structures, such as built-up top chords and columns of steel, is still more limited. If anything whatever is known about the results of tests, it is known that a few experiments would be inadequate, if not misleading. Hundreds of full-sized bars and members must be tested before such grades and forms can be determined as will approximate to the possible economy in bridge construction. This means the expenditure of many thousands of dollars. No bridge engineer, no bridge-builder, can afford such experiments, and it is unlikely that any railway or town corporation will undertake them. If an engineer does undertake them, he cannot spend the $50,000 or more necessary to get complete results, but the $5000 worth of testing he does buy is fairly his own. The

next engineer spends another $5000 in substantially the same direction; the next spends another $5000 in a collateral line of investigation, and so on; and if a hundred engineers and corporations should thus spend half a million of money without an organized co-operation, they would be travelling the same ground over and over again, and three-quarters of the money would be wastefully expended.

If, on the contrary, the government should provide a tenth part of this sum—$50,000—to buy material, and make structures, and systematically test them, under the superintendence of a board of engineers representing the different branches of construction, and also the manufacture and manipulation of iron and steel, is it not probable that every one of the bridge-builders and corporations in the country would get vastly better information, and that the whole science of construction would be at once lifted to a higher plane? And if twice this sum, which would then be paltry as measured by the results, were thus expended every year, might we not confidently look for revolutionary improvements in the following directions?

1. The intrinsically ridiculous factor of safety of six to one, half of which, at least, might be called the factor of ignorance—this enormous excess of material which loads down bridges with their own weight, and often exceeds the elastic limit of corporation finances—this dreadful incubus could be so largely removed that the same money would span twice the space.

2. Despite the so-called factor of safety, bridges tumble down every year, slaughtering hundreds of people, and involving enormous expenses. The damages alone for the Ashtabula bridge disaster have already reached three-quarters of a million dollars, and the case is not settled yet. Boilers also continue to explode, and ships to spring aleak at mal-constructed seams. Machinery in vessels, on railways, in works of all kinds, breaks in pieces, killing, delaying, bankrupting; the floors of great factories and theatres plunge down among broken columns, torturing and killing men and women in their *débris*. Is it not probable that the tenth part of the money damages paid for these disasters, if expended in the means of prevention indicated—in the thousands of experiments which would establish a law of fabrication and construction—is it not certain that it would very largely reduce this record of bankruptcy and death?

3. What an enormous impetus a positive knowledge of the strength of metals and structures under working conditions would give to construction in old, and especially in new, directions; to manufactures and to general business! Engineers and mechanics

naturally and properly employ the new steels and bronzes very sparingly and cautiously, until they know just what their physical properties are, and whether or not they can be uniformly produced. To supply this information, both to the makers and users of metals, by means of a comparison of chemical analyses with large-sized mechanical tests, is just what the present Board had organized and successfully begun. But the Congress of the United States, the only body which can practically sustain such a system of experiments, does not feel authorized to spend money in this most helpful direction to the people of the United States. It can spend millions on stone forts and cast-iron guns, which are likely to afford the country a very limited defence, but it cannot prove the new metals, which, in the shape of armor, guns, and shot, would be a defence indeed. It can lavish untold sums in digging channels for vessels up the creeks of the coast, but it virtuously refrains from squandering the public treasure to make a safe pathway for the locomotive. It can erect monuments and museums; it can dot the land over with public buildings, which, if they are not beautiful, are at least magnificently costly; but it recoils from violating the genius of republican institutions by ascertaining how to make even its own buildings safe and strong. It cannot divert the funds of the people from legitimate channels, such as private claims, in order to promote class-interests, such as metallurgy and engineering, although above 250,000 tons of iron are put every year into the bridges on which the people travel—although a million dollars a day were spent during two prosperous years on the iron work of American railways—although the government itself ordered 8000 tons of iron and steel supplies in fifteen months in the one department of public buildings, not to speak of public defences.

At the risk of wearying you with this subject, I feel it but just to the United States Test Board to give in this connection some account of its labors. The testing machine set up cost the government $35,000, but it cost the contractor over $100,000. The Board was authorized to spend $15,000 for its own expenses; it did spend $2248.79. All the rest of the appropriation it devoted to a series of experiments which will be referred to. In addition to this, one committee of the Board has collected and expended in experiments $1475, from iron and steel makers by passing round the hat. The three civilian members of the Board, excepting the secretary, have never received any pay for their services, and I know that they are together out of pocket in this business more than $5000. The Board has been

warmly aided by many engineers and others interested in its work, but, in the struggle with the Congress of the United States, the professional societies and the metal makers and users of the country have not given that united personal aid upon which success can alone depend. The Board has been embarrassed, and, finally, killed, by the misrepresentations of certain writers and Congressmen, and by the unfriendly action of other members of the government, not to speak of a general want of faith and interest in its labors. The very delay that corrected errors, developed improvements, and made the testing machine as perfect as it is, has been used as a powerful argument against the existence of the Board; and this, despite the following well-known and significant fact: The United States Board appointed to learn the causes of the bursting of steam-boilers, had an appropriation of $100,000. After spending $60,000 of it, the Board reported that its results were entirely unreliable, because it could get no gauges on which it could depend. As an example, at Pittsburgh, the gauges varied 150 pounds on a pressure of 300 pounds per square inch.

Meanwhile the Test Board has already made a large range of investigations, and worked out and tabulated the results. A part of these are already in print and will soon be distributed. A complete chemical laboratory has been set up at the Watertown Arsenal, and Mr. Andrew A. Blair, late chemist to the board, has made 213 complete analyses of irons and steels, and 249 of alloys. His report on his methods, already published, is a valuable contribution to science.

Commander L. A. Beardslee, U. S. N., has, with some aid from other members, completed and got into print the most exhaustive and important series of experiments ever made on chain cable, and on wrought iron generally. I had the honor, at the meeting a year ago, of presenting an abstract of these results to the Institute. The testing machine of the Navy Department was approximately adequate for these purposes. Over 2000 tests were made in this machine, besides a great number on piling, rolling, and reheating in various iron works. It was proved for the first time that the strength of wrought iron, and its welding power by ordinary methods, are varied more by the amount of its reduction in rolling than by ordinary differences in chemical composition. The unsafety of the admiralty proof tables for chain cable was demonstrated, and new tables were prepared which will be of the highest value to the navy and to the merchant marine.

Prof. Thurston has made and worked up the most complete series of experiments on record concerning bronzes; they are accompanied

by full analyses, and by an abstract of the preceding experiments at home and abroad on this subject. They are about to be issued with the above-mentioned reports. The equally complete series on other alloys is nearly finished.

Chief Engineer David Smith, U. S. N., has made an elaborate series of experiments on tool steels. They are not yet fully worked up, because the Navy Department refused the request of the Board to give him the necessary time. Actual tests by turning, boring, planing, slotting, and chipping, were made on 70 bars from 11 American and 3 English steels of the best brands. These are accompanied by 108 tests of the steels in tension, torsion, and compression, and by full analyses.

General William Sooy Smith has made, but not yet fully worked up, some important tests of beams.

Another committee of the Board has nearly completed a preliminary series of experiments on structural steels; they are not expected to be exhaustive, but they will be of much value in practice, and of perhaps more value in pointing out the direction of further and large-scale experiments, to determine the effects of chemical ingredients upon physical properties. One hundred and twenty-three specimens have been tested in tension, 190 in torsion, and 148 more are in hand. There are complete analyses of all the steels. These reports on alloys, tool steels, beams, and structural steels will be presented to the next Congress, and will probably be published.

The other members of the Board have rendered such services as they could, but the work of the committees which they have specially in charge could not be forwarded without money and apparatus.

One word for Mr. Albert H. Emery: To his engineering talent, mechanical culture and painstaking fidelity; to the patient devotion of all his energy, and more than all, his money, we are indebted for a marvel of invention, of development, of workmanship, of efficiency.

If the members of this Institute believe that the United States Government ought to provide money to realize the great possibilities of this machine, and to revolutionize the constructive arts, they should vigorously, and above all things, unitedly, appeal to the next Congress to appropriate enough for thorough work, and to appoint a suitable mixed commission to superintend its systematic expenditure.

DISCUSSION.

Dr. R. W. Raymond, of New York: Every member of the Institute will share the interest with which we have listened to Mr. Holley's admirable paper; and although the nature and rules of our society do not permit it to take action as a body on such questions, I think all the members would be glad, as individuals, to sign a memorial to Congress, asking that this important work may be continued, in the hands that have so well begun it. At the same time, I may be pardoned if I venture a word in behalf of those gentlemen who have been made, no doubt, with some justification, yet not, I think, with entire and impartial justice, the objects of Mr. Holley's eloquent and witty sarcasm—I mean the members and committees of Congress. In an experience of many years, during which it was my duty to present annually the claims of a semi-scientific public work, I found these gentlemen to be courteous and candid listeners, anxious to understand the merits of the questions they had to decide, but above all, universally and invariably *tired*—besieged from morning to night by the advocates of schemes for various enterprises, each one of which was pressed upon them as the immediate necessity of an imperilled country. Inevitably, for sheer lack of time to study and choose among these propositions, they are thrown into the habitual attitude of hostility to anything that calls for money. This is particularly the case with regard to appropriations for work outside of the regular executive departments. Every such plan is required, I think, very properly, to be demonstrated to be properly within the sphere of governmental action, and at the same time beyond the duty and the capacity of existing governmental agencies. And this demonstration must be made, as I have said, to men who do not thoroughly understand the subject beforehand, and who are plausibly assailed with similar arguments all the time. If there is (as just now there certainly is), special pressure for economy in expenditures, the question at once arises, with regard to a number of schemes, all proved to be useful and necessary, which of them can wait. And finally, when any special work, statistical or scientific, depends largely for its value upon its continuity, the difficulty arises that the men whose influence and votes inaugurated the work, pass out of office, and their successors need fresh instruction as to its importance.

We cannot help these things, they are necessary incidents of our form of government. Moreover, it might be shown that they have

their compensations. But, not to enter upon that view, we may easily see, that on the one hand, the difficulty of getting systematic and continuous work by special commissions adequately supported by our government, should lead us to rely as little as possible upon legislation for such purposes; and, on the other hand, when, as Mr. Holley has shown in the present case, the work is legitimate and necessary, we should be both patient and frank in urging its claims. There has been a great deal of disingenuous argument both in the States and at Washington, to influence legislatures in behalf of public and scientific enterprises. Appropriations have been obtained as "entering wedges," on the idea that if the legislature were told how much time and money would really be required in the end, it would refuse to begin at all. Surveys, public buildings, and investigations of far-reaching importance have been inaugurated in a small way, so that the legislature did not know to what it was committing itself. In the present instance, this error was not committed; but the United States Test Board now suffers as the result of numerous instances all over the country; and we must simply continue patiently and frankly to remove misconceptions concerning it.

THE GREAT BLAST AT GLENDON.

BY ELLIS CLARK, JR., GLENDON IRON WORKS, EASTON, PA.

During the winter of 1877–78 the Glendon Iron Company, by the advice of the superintendent, Mr. Frank Firmstone, decided to make the experiment of exploding a heavy blast of gunpowder in their limestone quarry at Cedar Hill, Palmer Township, Northampton County, Pennsylvania. The amount of limestone used in the five furnaces of the company is about 60,000 tons per annum, and it was thought that with successful results, this entire amount or more would be thrown down at a single operation, disintegrating the mass to a greater or less extent, and obviating the necessity of blasting and boring the rock from the face of the cliff, both of which operations are slow, and therefore, costly.

The rock at the quarry is Auroral limestone, or No. 2 of the old Survey. No fossils have ever been found in the quarry, nor in the immediate vicinity. The nature of the rock varies considerably in

the different parts of the quarry, and changes to some extent as each additional stratum is opened. In the northeast face it is of a good quality, blue in color, and of a moderate hardness. In the face of the new quarry (the eastern portion of the quarry was begun several years later than the western, and is the portion which was operated on in the great blast), some of the strata are of a poorer quality, being slaty with dirt seams, but not so much so as in some portions of the old quarry, where, though the rock itself is very good, there is so much dirt in the seams and fissures as to make the removal of it quite an item in the quarry account.

On the southwest face of the old quarry the rock is hard, blue, and a little flinty. Some of the more slaty rock contains so little lime that it is useless for fluxing purposes in the furnace. The amount of dirt, waste, spalls, etc., is quite small, being probably less than 5 per cent. of the amount of stone obtained.

In the research for precedents it was found that the principal blasts, resembling the one at Glendon, were made at Dover and Holyhead, and at other points on the English coast. A few in other parts of the world were also noted. In the case under consideration, methods of mining were of more importance than the quantity of powder used, which was finally determined without reference to any of the recorded cases.

In the examination, however, all facts bearing on the subject were recorded, and it was found that the amount of powder used varied with the cube of the line of least resistance in almost every mine exploded.

That the force of exploded gunpowder bears some definite relation to the line of least resistance, has been known for over a century, it being the law which both Vauban and Belidor were led to assume as the result of their investigations. The most definite information on the subject was found in the writings of Captain Hutchinson of the English Royal Engineers, who was engaged in a number of extensive operations of a similar character to the one proposed at Glendon. One of the most complete descriptions from his pen was the "Account of the Demolition and Removal by Blasting of a portion of the Round Down Cliff, near Dover, in January, 1843," in the *Professional Papers of the English Royal Engineers*, vol. vi. Space forbids the quoting of the various authorities consulted on the subject, and as the objects of the research were to find the nature and cost of the preliminary mining work done, the quality, hardness, and tenacity of the rock operated on, and the coefficient by which

the cube of the line of least resistance was divided, a few of these items will be cited.

At one of the operations at Holyhead, an entrance gallery 5 feet 6 inches by 3 feet 6 inches was first driven into the face of the rock, a hard quartzite schist, to a distance of 34 feet, when a shaft 3 feet 6 inches by 3 feet 6 inches was sunk to a depth of 14 feet; from this level galleries were driven some distance right and left, with four short headings at intervals returning towards the face of the rock, and terminating in chambers for the charges, which were 3 feet below the level of the quarry. The four charges, amounting in all to 12,000 pounds of powder, were inclosed in canvas bags coated with tar. They were calculated at the rate of 1 pound of powder to 3 tons of rock. For tamping a red clay was used, it was well rammed up close to the bags of powder, leaving a small space around these and continued to the mouth of the gallery. The charges were fired simultaneously by a Grove's battery. The total quantity of rock removed was about 40,000 tons; it was separated into various-sized blocks.

In another blast at the same quarry, in January, 1867, the method of calculating the amount of powder was as follows: The cubical content of the mass to be dislodged was divided by 12, the maximum number of cubic feet per ton, and the quotient by 5, it being estimated in this case, that 1 pound of powder was required to dislodge 5 tons of rock. The length of the face of the rock being 210 feet, its height 115 feet, and the horizontal depth to be removed 40 feet, the proper quantity of powder was, therefore, in round numbers, 16,000 pounds. The quantities applicable to charges Nos. 1, 2, 3, and 4, the lines of least resistance, being respectively 26, 25, 20, and 27 feet, were 4200, 4500, 2300, and 5000 pounds. That these estimates were very nearly correct appears from the fact that the force of the powder was mainly expended in displacing and breaking up the rock, but little concussion of the air being produced. The report of Col. Servante of the Royal Engineers, who was sent to witness the explosion, says, the mass was quietly overthrown down to the level of the quarry ground line with very little noise, and scarcely a stone was thrown into the air. The quantity of rock detached was found to be 120,000 tons in blocks of from 3 to 40 tons, averaging 7½ tons of stone to 1 pound of powder.

The operations were conducted by Mr. C. G. Reitheimer, the engineer employed by the Messrs. Rigby, the proprietors of the quarry. The galleries and shaft were tamped with clay, and the tamping was extended through the entrance gallery to the surface of the rock.

In the large blast of sandstone at Lime Point entrance to San

Francisco Bay, the lines of least resistance being between 45 and 50 feet, the charges were 4000 pounds and 3500 pounds, or $\frac{1}{28}$ LLR^3.

In the three large charges used to blast Round Down Cliff, near Dover, in which the material was chalk, the amount of powder was calculated at $\frac{1}{32}$ LLR^3.

In India the proportion of powder was much increased in heavy blasts, as appears from the article "On the Tracing and Construction of Roads in Mountainous Countries" (Hindostan and Thibet Road), by Major James Browne, R. E., where the proportions successfully used were

For granite and gneiss, $\frac{1}{8}$ LLR.^{3}

For limestone or hard sandstone, $\frac{1}{10}$ LLR^3.

For conglomerate or slate shale, $\frac{1}{12}$ LLR^3.

The result of these charges was, that each pound of powder could be expected to blow out about 115 cubic feet in limestone, but not more than 75 to 85 cubic feet in granite.

Mr. George Robertson, in the *Civil Engineer and Architects' Journal*, vol. xxiv, Feb. 1861, gives a number of instances of heavy blasting at Holyhead and other places. To avoid blowing the tamping the rule was that the shaft should have $\frac{1}{3}$ less grip (or resistance from face) than depth, as a maximum, and the depth was often twice the line of least resistance (LLR) or more. In a mine fired November 13th, 1850, the LLR was 21 feet; 600 pounds of powder were used, and 3000 tons of stone were obtained, or 5 tons of stone to the pound of powder; this gives a charge of $\frac{1}{15}$ LLR^3. From experience it was found that 30 feet was about the economical length for the line of least resistance. The rule at Holyhead for ordinary gangways was $\frac{1}{15}$ to $\frac{1}{20}$ LLR^3, and for ordinary shafts $\frac{1}{12}$ LLR^3. In re-entrant angles or corners the charge was increased to $\frac{1}{10}$ LLR^3.

Blast at Turner's Granite Quarry, from *Civil Engineer and Architects' Journal*, vol. xv, page 400. Charge, 3 tons of gunpowder, deposited in 2 charges or chambers, 1½ tons in each. Shaft 60 feet deep, chambers 17 feet long, 7000 to 8000 tons of rock blown down.

A blast was fired in hard Basaltic rock at Down Hill, in 1846. The charge was 2500 pounds, and the line of least resistance 50 feet, which gives $\frac{1}{50}$ LLR^3 for the charge. It is not stated whether it was a success. The charge was extremely small for so great a line of least resistance.

A study of the charges and their results at Holyhead, shows a gradual increase of the relation of the charge to the cube of the line of least resistance. The first was $\frac{1}{25}$ LLR^3, or nearly the same for the hard quartzite rock of Holyhead, as had been used for the soft

chalk of Dover a few years previously, where $\frac{1}{26}$ LLR^3 was the proportion; this charge was gradually increased to $\frac{1}{15}$, $\frac{1}{14}$, $\frac{1}{11}$, $\frac{1}{9}$, and $\frac{1}{8}$, and in exceptional cases as high as $\frac{1}{3}$ or $\frac{2}{7}$.

In the later blasts at Holyhead, a high proportion, $\frac{1}{4}$ LLR^3 was used, and in India $\frac{1}{10}$ LLR^3 was recommended for limestone, and $\frac{1}{8}$ LLR^3 for granite. In view of all these precedents for high charges, and as the operations in India on the Hindostan and Thibet Road were considered as approaching more closely the work to be performed at Glendon than any of the others, the relation to $\frac{1}{10}$ LLR^3 was adopted, which, with the various lines of least resistance, would give the amount of powder to be used in round numbers, 16,000 pounds.

Captain Raymond, Engineer Corps, U. S. A., an expert in the matter of large blasts, gave as his opinion, that the charge of 16,000 pounds was entirely too high, and recommended 8000 pounds, this being somewhat more than that given by the Lime Point blast rule $\frac{1}{28}$ LRR^3, which explosion Captain Raymond had witnessed. But since an excess of powder could do no harm, while a deficiency might cause a total failure, the quantity was finally fixed at 12,000 pounds, being a mean between that given by Browne's rule, and that recommended by Captain Raymond.

The work of the blast will be described under the following heads: Survey and map; mining work, comprising driving the gangways, sinking the shafts, and opening the chambers; electrical experiments and tests; placing the powder; tamping; firing; statement of cost of blast and percentages; results.

SURVEY AND MAP.

By the survey the quarry was ascertained to be 200 feet wide, from 130 to 140 feet in perpendicular height, and 550 feet in length, this being the result of about 20 years' working. The line of the new quarry, as is seen by the accompanying map, projected about 100 feet beyond that of the old, and it was this point of rock upon which the blast was to be made.

The results of the research had shown the method pursued at Holyhead to be preferable to any other in the mode of excavation, and dimensions of drifts and shafts; it was therefore adopted in general, but modified by the peculiar circumstances of the projected work.

A careful examination was made of the south side of the new quarry, in order to find the most favorable place for the opening of the main drift; its various positions were studied, and it was finally located in the corner where the face of the old quarry touches the

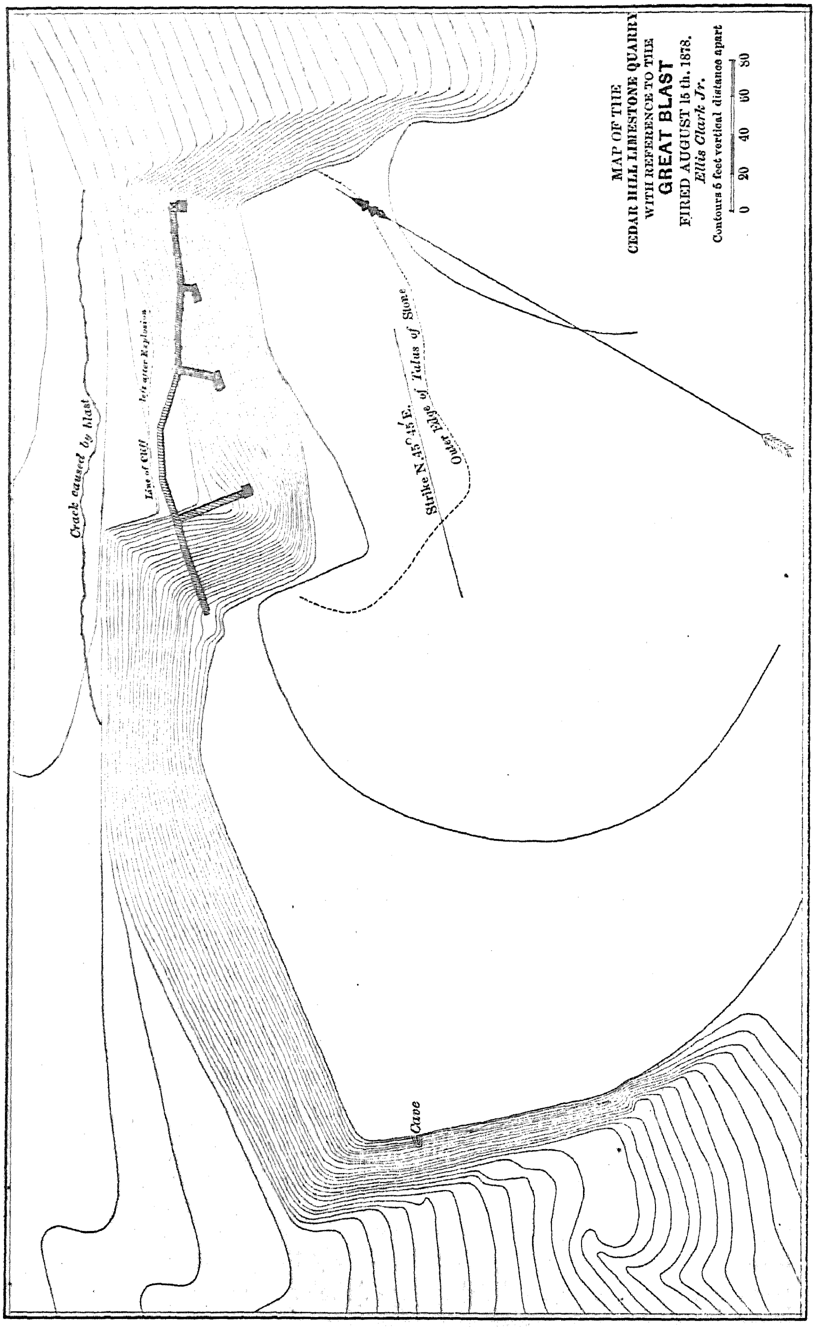
side of the new, in a seam of broken rock and dirt, which had apparently been caused by two beds of solid and massive limestone meeting in an inverted V along a perpendicular plane, and making a nearly horizontal junction. In the south side of the old quarry is a cave formed by two beds meeting in a similar way. This cave, though not in range with the position selected for the mouth of the gangway, according to the strike of the rock, which as nearly as can be ascertained is N. 40°. 15′ E., was supposed to be a continuation of the same fissure or fault, its precise nature being difficult to ascertain, but being probably the result of a violent flexure in connection with a sliding fault.

The main gangway being the longest, the reduction of its cost to the minimum was necessary to the pecuniary success of the blast, and this, it was affirmed, could be accomplished if the seam when followed was continuous. As none of the stratification planes coincided with the fissure, the chances were that the seam, if selected, would not be found continuous, although there was a possibility of its being so. The advantages adduced by those in favor of following the fissure, were the great difference in cost, which would probably have amounted to as much as $4 per foot, or nearly $1000 in the whole length of the gangway. It was asserted at the time, and subsequently found to be the case, that the cave at the south side of the old quarry was actually a portion of the fissure under discussion, and that the fault had extended entirely across the face of the old quarry, but had been blasted away. It was finally decided to adopt the fissure as the line of the main gangway, and follow it to the end.

MINING WORK.

Work was begun on the main tunnel by two drillers taken from the quarry, and an open cut was made by them about seven feet in length along the face of the old quarry, the loose rocks before the proposed mouth being thrown down the bank. The main difference between the plan of mining adopted and that used at Holyhead and elsewhere was that the shafts, instead of being sunk directly from the main gangway and the cross-cut gangways driven from the bottom, were sunk from the ends of these cross-cuts, thus obviating the necessity of handling the rock twice, and of hoisting all the material mined in the cross-cuts up the shaft. By this economy at least 50 per cent. was gained in the driving of the cross-gangways.

The main gangway was begun at a height of about fifteen feet above the general surface of the quarry, so as to give some grip to


MAP OF THE
CEDAR HILL LIMESTONE QUARRY
WITH REFERENCE TO THE
GREAT BLAST
FIRED AUGUST 15 th. 1878.
Ellis Clark Jr.
Contours 5 feet vertical distance apart
0 20 40 60 80
Crack caused by blast
Line of Cliff left after Explosion
Strike N. 45° 45' E.
Outer Edge of Talus of Stone
Cave

the tamping in the shafts. After this gangway had been driven its entire extent, the position of the powder-chambers was marked upon the plan, and cross-gangways were driven to a point a little to the right, left, or behind them; this was done so that the powder-chamber would be by itself and not merely an enlargement of the bottom of the shaft. The shafts were then sunk to a depth of two feet below the level of the floor of the quarry in the direction of the line of least resistance, and the chambers were started and excavated to the required size. The object of this slightly complicated system of tunnels, shafts, and chambers was to cause the tamping to gain as much resistance as possible to the force of the explosion, by the increased resistance caused by the friction of three right angles. As the expansive force of the powder would bear upon the tamping, the first resultant would act directly across the shaft in a horizontal line; the second would act up the shaft in a vertical line, while the third would act back along the cross-cut tunnel to the junction with the main tunnel, where its almost expended force was received against a buttress of 8-inch square timber which fitted into hitches cut in the solid rock for their reception on the opposite side of the gangway.

This was the plan as originally proposed, and which in a general way was carried out, the modifications being caused by the change in direction of the main tunnel, which will subsequently be noticed. The driving of the tunnels, etc., was given out by contract, as this has been found decidedly the most advantageous way of working; tools, oil, explosives, and fuse were provided at the company's expense, the labor alone being furnished by the contractors. No charge was made for sharpening tools, and when the second gang of men was started, and the systematic ventilation of the workings begun, the drills, picks, etc., were carried by the blower boys to and from the blacksmith shop, so that no time was lost in this way by the miners.

From the beginning of the work, a day and night shift was employed. Two miners took the contract, hiring two laborers; one of the miners with a laborer worked on the day shift, and the other with his laborer on the night shift, alternating every week. It being contract work, the management did not interfere with the number of working hours, which were from 7 A.M. to 5 P.M. with one hour at noon, or nine hours per day. On Saturday work was stopped at 3 P.M., making seven hours' work, and there was no night shift. The hours presumably worked by the night shift were from 5 P.M. to 3 A.M., with one half hour for a meal, but these working hours

were by no means strictly observed, as after the tunnels had reached the solid rock, quitting-time was governed by the holes fired, work being stopped after the final blast for the day or night, to allow the powder-smoke to be driven out before the following gang came to work. The width and height of the tunnels and dimensions of the shafts were left to the miners, as experience has shown that the size most convenient for working (3 feet wide by 5 feet 6 inches high), is quite large enough for all subsequent requirements.

The stratification and cleavage of the rock, in the face of the main gangway along the line of fissure or fault, changed every few feet, the rock generally remaining soft enough to work with the pick, and was in some places so loose as to be handled with the shovel. The first 25 feet of the main gangway were taken at the price of $3.00 per yard, and the remaining distance was taken at $2.75 per yard.

At 200 feet the ventilation had become very deficient, and to remedy this, a winnowing fan, driven by a treadle, was placed at the mouth of the gangway, leaving sufficient room for the passage of the wheel-barrows. A four-inch galvanized iron pipe led away from the fan along the straight portion of the gangway for a distance of 80 feet, then 4-inch ordinary stove-pipe was used for the remainder of the distance, about 120 feet. These pipes were supported from the roof and sides of the gangway, either by wires to the timbers in the roof, or to large flattened horseshoe nails with an eye through the head, called "spuds." These were either driven into cracks in the rock, or into pieces of wood wedged into these cracks. A pressure of ½ inch of water was obtained at the mouth of the machine, but much of this was lost in its passage through the pipes.

At the distance of 237 feet 6 inches, the long tunnel was finished. It was begun on February 19th, and finished on March 26th, in 56 ten-hour shifts. The average progress per 24 hours was 8 feet 6 inches, the greatest distance made in one day was 19 feet, and the least 3 feet. The main gangway was driven so cheaply that it can by no means be regarded as a criterion of the work. Under ordinary circumstances, supposing the rock to have the same average character as that in the quarry, and the gangway to be driven along its strike, which renders the working less expensive than if driven across the strike, the tunnel, instead of costing $1.13 per foot, and a total of $262.05, would have probably cost at least $4.00 per foot, and a total of nearly $1000. After the main gangway was finished, the position of the powder-chambers was laid out on the map of the quarry by Mr. Firmstone.

These were laid down at distances of about 45 feet apart, and about 50 feet from the position of the face of the quarry at that time.

No. 1 cross-cut tunnel was at right angles to the position of the main gangway, and at a distance of 56 feet from the mouth. After the loose rock that lined the side of the main gangway had been penetrated, the rock was moderately hard and became harder as the gangway advanced towards the face, finally becoming so hard that a price of $8 per foot, the highest price on the work, was given the contractors to penetrate a stratum of very hard, compact blue limestone. At about this point the air, which had been kept moderately good by the wheelers stopping and giving a few turns to the fan at each trip, became so bad from the continued blasting necessitated by the hardness of the rock, that two boys were employed to blow it; the instructions given them were to keep the air good at all times, and when not needed at the blower, to help the contractors in various ways, such as wheeling out the broken stone, and carrying drills to and from the blacksmith shop.

The progress of the work was so much retarded by the hard rock in No. 1 tunnel, that an additional gang of two contractors and their laborers was set to work sinking No. 4 shaft, which was situated at the extreme end of the main gangway. At first the work, being carried on in the loose rock of the floor of the gangway, progressed very rapidly, but at a depth of 4 feet a solid floor of rock was met, which reached entirely across the shaft, cutting off the fault completely. This floor was quite level and smooth, the rock of which it was composed being of a moderately hard nature. A feature of this shaft was the recurrence of the floors, which were probably planes of stratification, and a portion of the bottom of the large synclinal, which distinctly shows itself in the south side of the old quarry. These floors rather expedited the work, as the rock parted along them much more easily than where they were absent. No water was found till a depth of 12 feet was reached, when the walls became damp; and at a depth of 15 feet water was struck in a drill hole. The water was evidently under considerable pressure, being ejected as from a fountain, when the drill was withdrawn, covering the floor of the shaft in a very short time, and flowing at the rate of $2\frac{1}{2}$ gallons per minute. From the character of the rock, which is noted for the small amount of water found in it, and from other circumstances, it was not supposed that the water came at this rate from a regular spring, but that some cavity had in time become filled with water, which, once withdrawn, would not immediately be refilled. During the sinking of the shaft water came in it from the sides and the floor in small quantities, not amounting

to much more than four or five gallons per hour, or barely enough to inconvenience the work. This water was hoisted in buckets, but it was not found necessary to take it to the mouth of the tunnel to empty it, as the bottom of the gangway, a short distance from the shaft, was sufficiently broken to absorb the water in moderate quantities, little if any of which found its way back to the shaft.

The depth of No. 4 shaft was 24 feet and the dimensions of the powder-chamber 5 feet square by 6 feet long. The shaft was begun April 15th, and the powder-chamber finished May 20th, in 56 shifts. The bottom of the shaft, and the powder-chamber, which were situated at a depth of 2 feet 6 inches below the level of the quarry at the face opposite, was formed by one of the floors previously mentioned. The depth required was 2 feet below the ground line of the quarry, but the floor made it easier to remove the rock down to its level.

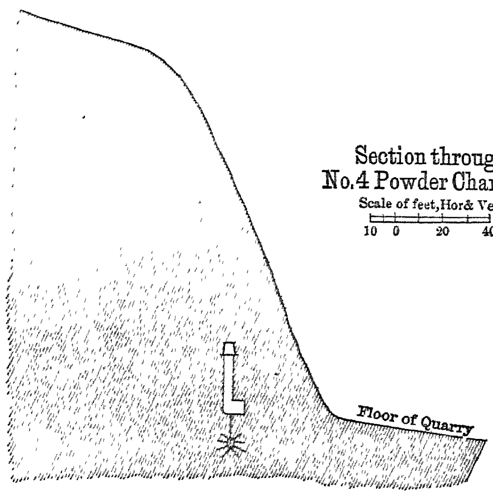
At this depth, 24 feet below the level of the gangway, a small quantity of water still oozed up from the bottom, and trickled down from the sides, but even this small amount would have been sufficient to spoil the entire quantity of powder placed therein, if no precautions were taken against it. In the endeavor to rid the shaft of this water, a two and a half inch drill-hole was sunk in the bottom of the shaft, in the hopes of meeting some crack or seam which would carry off the water. The chances of finding an absorbing seam were perhaps a little greater than of finding a flowing stream, although the latter could be managed by plugging up the drill-hole, but at a depth of 14 feet no crack was found, either absorbing or otherwise, and so it was determined to make one.

To this end, 10 cartridges, about 6 pounds of giant-powder or dynamite, were dropped into the hole; some of these were left in the original cartridge-paper, tightly rolled, while others had their tops opened. Two long waterproof fuses were inserted into exploders, and tied up tightly in two cartridges, which were made as nearly waterproof as possible with grease and tallow. All the cartridges were then covered with sand, and above this was about two feet of water. The fuses were lighted, and in a few minutes a dull thud was heard at the mouth of the gangway, and a few loose stones were rolled from the face of the quarry opposite, thirty-five feet distant. On removing a little of the sand with a scraper, the remainder suddenly descended into the hole, being followed by all the water remaining in the bottom of the shaft, proving that the explosion of this comparatively heavy charge of giant powder had opened below a seam sufficiently large to carry away all the water, thus making

of the drill-hole an absorbing artesian well. To protect the drill-hole from being choked, and also to keep the bottom of the powder from the damp floor of the chamber, the shaft and powder-chamber were paved with "cobble stones" set on edge, and wedged tightly against each other. These were then covered to the depth of a few inches with smaller stones, which subsequently supported the sand used in tamping without allowing it to choke up the drill-hole.

No. 1 shaft was sunk to the depth of 18 feet through moderately hard rock, and was perfectly dry. At this depth the powder-chamber was opened, being made 5 feet 3 inches square and 6 feet long. It was parallel to the direction of the gangway.

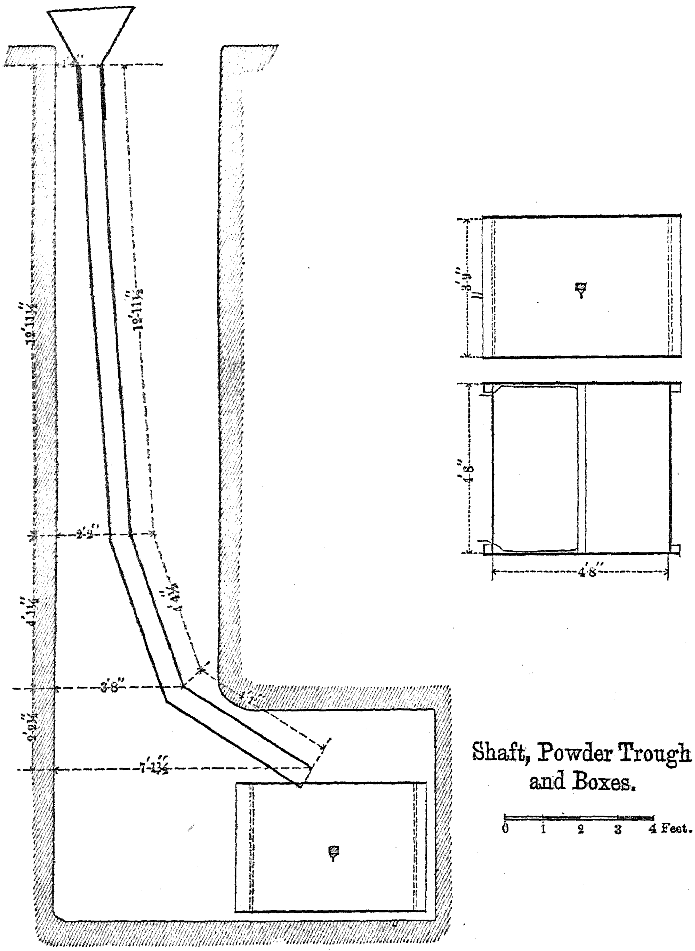
No. 2 cross-cut was started at the turn in the main gangway, its direction being nearly normal to the curve; it was driven 25 feet 6



inches through rather hard rock. The shaft sunk at its end was square, and 19 feet 6 inches deep. It was excavated with a twist in it, so that the powder-chamber which was to start from one of the sides of the shaft would have its sides parallel to the face of the quarry. The rock in No. 2 shaft and powder-chamber was quite soft, and in places could be easily worked with the pick.

No. 3 cross-cut was 10 feet long, most of this distance being through a soft, earthy stratum. The shaft sunk at its end was through firm rock, and had an intentional twist given it for the same reason as No. 2. No. 3 powder-chamber concluded the mining work. It had been at first expected that No. 2 would be the last completed, but an extra shift of men were put on in the gangway and shaft, and

this, together with the softer nature of the rock, caused the work to proceed more rapidly than in No. 3. After No. 3 chamber was finished, the miners were kept a few days longer cleaning up, drilling holes for the plugs to which the copper conducting wires were to be fastened, cutting hitches in the rock at the west side of the main gangway opposite Nos. 1 and 2 gangways for the support of



Shaft, Powder Trough and Boxes.

the buttresses, and wheeling in loam to cover the floor of the gangways for the double purpose of burying the wire for its protection, and of lessening the probability of a spark being made by the hobnails of boots striking against the rocky floor, when the men were engaged in passing the powder. For this purpose the gangways and

cross-cuts were all covered to the depth of about 4 inches with loam, which served, moreover, in part for tamping. The accompanying cuts are sectional views of the shaft, powder-trough, powder-chamber, and boxes.

EXPLOSIVES.

At the beginning of the work, ordinary *F F* blasting powder was used, but when the hard stratum of rock in No. 1 cross-cut was reached, and a progress of but 5 or 6 inches a day made, it was determined to try giant-powder or dynamite for the purpose of expediting the work.

Its fumes had the usual effect for the first few days, seriously affecting every one inhaling them. One of the working gangs used giant-powder exclusively while they remained on the work; the other not liking its effects used the ordinary powder till towards the expiration of their contract.

The percentage table, following, makes a very unfavorable showing for giant-powder compared with blasting powder, raising the cost of explosive, from 7 and 8 to 20 and 21 per cent. It, of course, diminished the labor percentage in the same proportion, but giant-powder at 40 cents per pound, and caps at $1.50 per box, are more matters of economy than labor at $1 per day.

The cross-cuts and shafts in which giant-powder was used made a little better headway than the others, and the giant-powder was found very efficacious in cutting out the corners in the chambers; but taken as a whole the experiment with giant-powder was not a success.

ELECTRICAL EXPERIMENTS AND TESTS.

The mines were fired by galvano-electricity, furnished by a 30-cell bichromate of potash battery. Laflin and Rand copper exploders were used. The tests and experiments were carried on by F. C. Blake, Adjunct Professor of Chemistry at Lafayette College. A differential galvanometer, bought of L. G. Tillotson & Co., was used in connection with a rheostat from the same manufacturer.

The battery used in testing the exploders and connections while the powder and tamping were being placed, consisted of a single Daniell cell, 7 inches high and 3½ inches in diameter, filled about one-fourth its height with water. This gave a feeble current, sufficient to show on the galvanometer, but by no means strong enough to heat the platinum wires in the exploders to the firing-point, as was found by actual trial before the powder was placed in position.

As each exploder in the previous experiments had shown about .80 ohm resistance, this gave 6.40 ohms for that of the eight exploders, the heavy copper wire, 62 thousandths of an inch in diameter, gave 2 ohms resistance, and about 1 ohm of resistance was caused by the imperfection of the connections, making 8.40 ohms by calculation. The galvanometer and rheostat showed 9 ohms, the amount varying a little with the change of temperature caused by the time of day. Two exploders were used in each powder-box to fire the charge at two points, and thus hasten the combustion.

After the shafts and galleries had been tamped, and the buttresses erected, the wires from the main battery were let down from the crest of the quarry, and connected with the wires leading into the powder-chamber. Although 24 cells of the battery only were required, material enough was on hand to make a 30-cell battery, and so this number of cells were filled with the bichromate of potash solution, the internal resistance of the battery tested, and eight exploders fired simultaneously through a heavier resistance than was required.

The calculations of the resistance of the exploders and wire in ohms, the amount of electricity in webers required to overcome this resistance, and the internal resistance of the battery, were calculated according to formulæ given in Mr. Julius H. Striedinger's paper published in the *Transactions of the American Society of Civil Engineers* on "The Simultaneous Explosion of Thousands of Mines."

PLACING THE POWDER.

A few days before the powder arrived, the boxes for its reception were made and placed in their position in the powder-chambers. These boxes were calculated of a size sufficient to hold the powder placed in each chamber, and have a few inches of space remaining at the top. Holes were bored in the sides of the boxes through which the copper wires leading from the battery to the exploders were inserted, the wires were then led up each side of the shaft, and fastened to a wooden plug at the top, thence running along the cross-cut, where they were carefully buried out of the reach of damage in the loam that had been placed on the floor. In the portions of the main gangway which it was not intended to tamp, the wire was carried on plugs overhead or at the side. The powder-boxes, after being placed in position, did not touch the sides of the chambers by several inches, and for the purpose of saving time in the subsequent tamping this, as well as the bottom of the shaft to nearly the level of the top of

the powder-boxes, was filled in with sand. As it had been determined after mature consideration to pass the powder in from hand to hand along a line of men, it was thought advisable to put the men in training by passing the sand in the same way. Kegs holding 25 pounds of powder, with half-inch iron handles, as being most convenient, were used. Two kinds of sand were employed, dry and moist, the dry being placed immediately around the powder-boxes, between them and the walls of the chamber. The moist was used for filling the bottom of the shaft, where its dampness would not affect the powder.

Some of the sand used had been in store for several years, and was so dusty that in the rear portion of the main gangway, where the ventilation was bad, it became necessary to change the men every ten or fifteen minutes. In some places the dust was so thick that a miner's lamp was extinguished, and it was with difficulty that a sulphur match could be ignited.

Forty-seven men were employed, working under Mr. Valentine Mutchler, in charge of the quarry, and Adam Boonton, the head blaster. The sand was first passed into No. 1 chamber, that being the nearest, and 92 feet from the mouth of the main gangway; 74 kegs of moist sand, and 144 kegs of dry, in all 218 kegs, were passed in three hours. This included the delay of placing the men, and getting started.

In No. 2 shaft, a distance of 147 feet from the mouth, 84 moist, and 167 dry, in all 251, were passed in one hour.

In No. 3 shaft, a distance of 194 feet, 75 moist, and 178 dry, in all 253, were passed in in two hours.

In No. 4, a distance of 233 feet, 99 of moist, and 185 dry, in all 284, were passed in in two hours. It was at this shaft that most of the delay occurred incident to the frequent changing of the men on account of the dusty state of the air.

The dry sand that was to surround the boxes was poured down the trough through which the powder was to run, while the moist sand was merely thrown into the shaft.

Previous to putting in the powder, ladders were erected in all the shafts, and light round timbers placed across the gangways just back of the shafts. These latter were to keep the men, who would be working entirely in the dark while the powder was exposed, from falling into the shafts. It first had been the intention to use lights while the powder was being placed in the boxes, but this was given up as dangerous, and the experience of putting the sand around the boxes showed that the men could work almost as well in the dark.

The powder used was condemned Government mortar powder, and was delivered in a car on the Lehigh and Susquehanna Division of the Central Railroad of New Jersey in front of the quarry.

This powder was condemned because it had lost its glazing, and this, while impairing slightly, if any, its explosive power, made it so exceedingly dusty that no lights were used while it was being placed or remained uncovered. Warned by the experience of the dusty sand, aspirators were made out of wet sponge, and given to those men who would be sufficiently close to the powder to be inconvenienced by the dust, and experience subsequently showed that without them the powder could not have been placed in double the time.

Every precaution was taken to avoid accident, the consequence of which would have been fatal to all engaged. Among other precautions, water was sprinkled on the ground after the filling of each chamber to prevent the formation of a train of powder.

The powder, which arrived in 100 lb. kegs, was at first passed to the chambers in them, 50 lbs. of it being poured out to make its handling easier, but this plan taking too long, the powder was emptied into 25 lb. kegs, and passed in them from hand to hand to the shaft, where it was poured down the trough into the box below. A cloth was spread over the ground where the barrels were being emptied, to prevent the powder from scattering.

Forty-four men were employed in this operation, and No. 4 chamber was first filled. Some trouble was experienced in this shaft, as the powder at first did not run well down the trough, which had become slightly damp, but this was only temporary, and the box, holding 3500 lbs. of powder, was filled in 1 hour and 50 minutes. No. 3 chamber received 3000 lbs. of powder in 50 minutes, No. 2 chamber, holding 2700 lbs., was filled in 45 minutes, and No. 1, holding 2800 lbs., in 35 minutes.

The placing of the powder was finished at 5 o'clock Saturday evening, and as working on the following day was decided against, two trustworthy men were left in charge of the mouth of the tunnel, with two others to relieve them in the morning.

The tamping of the shafts was begun on August 12th with a force of 57 men. Sand was used, both moist and dry, the dry for filling in immediately around the powder-boxes, and the moist for the remainder of the shaft.

No. 1 shaft, a distance of 92 feet from the mouth, was first filled. 250 kegs of dry sand, and 1028 of moist were used, in all 1278, it

being completed in 3 hours and 50 minutes. This shaft was 18 feet deep, and like the others about $4\frac{1}{2}$ feet square.

No. 2 shaft, a distance of 147 feet, was filled with 249 kegs of dry sand, and 1070 of moist, in all 1319, in 6 hours and 30 minutes. This shaft was 19 feet 6 inches deep, and owing to the softness and frangibility of the rock, a little larger in its cross section than the others.

No. 3 shaft, a distance of 194 feet, was filled with 102 kegs of dry, and 869 kegs of moist sand, a total of 971, in 3 hours and 15 minutes. This shaft was 21 feet 3 inches deep.

No. 4 shaft, distant 233 feet from the mouth of the gangway, and 24 feet deep, was filled with 165 kegs of dry and 859 of moist, in all 1024, in 5 hours and 10 minutes. The tamping of the shafts with sand had thus taken almost 2 full days of 10 hours each. But one shift had thus far been employed on account of an insufficiency of men. No lights were used until the shafts were filled with sand. On Tuesday afternoon at 4 o'clock, the tamping of the gangways with loam was begun. In one hour, 6 lineal feet of No. 1 gangway, 4 feet of No. 2, and 6 feet of the main gangway were tamped. The force was divided as follows, twelve men being required for the inside work. The loam was passed up from the quarry level to the mouth of the main gangway (a perpendicular distance of about 16 feet), and emptied into wheelbarrows, of which 8 were in use. Six men inside were engaged in ramming the loam into a coherent mass, and the other six in wheeling it in. Some little study and care were required to ascertain the most expeditious method of wheeling, in so constricted a place as a gangway, three and one half feet wide. But at first three of the wheelers went to No. 4, two to No. 2, and one to No. 1, this last making double the number of trips, on account of the diminished distance. On Wednesday, August 14th, the loam tamping was continued, the force being divided into three gangs, working 8 hours each. The first contained 22 men, and worked from 6 A.M. to 2 P.M.; the second, consisting of 19 men, started at 2 P.M. and worked till 10 P.M.; and the third, of 17 men, began at 10 P.M., and worked till 6 A.M. The men were disposed as already stated, 6 wheeling and 6 shovelling and tamping, while the remainder, 5 to 9, helped to unload the cars, wheeled limestone spalls, and passed the kegs up the bank. It was soon observed that the tamping of the main tunnel from No. 4 shaft, governed the time of the entire work, and so matters were expedited in that direction at the expense of the others, as much as possible. Nos. 1 and 2 were finished to within about 8 feet of their mouth, leaving room sufficient for two barrows,

and the main gangway was then pushed harder than ever. At 2 P.M. on Thursday, August 15th, the main gangway was tamped to a distance of 25 feet back of No. 3 cross-cut. Nos. 1 and 2 had been finished shortly before. Immediately after the tampers had withdrawn, the carpenters fitted 8-inch timbers between planking laid on the tamping and the hitches in the solid rock that had been cut previously. The electric tests had been made at intervals of 10 or 15 minutes during the entire time the powder and tamping were being placed.

FIRING.

While the buttresses were being erected the battery was placed in position. The battery house was situated on Cedar Hill, at a distance of 300 feet back from the face of the quarry, and was about 400 feet in a straight line from the powder-chambers. The battery, consisting of 30 bichromate of potash cells, was connected with the wires leading down the cliff, through the gangways and into the powder-chambers.

Outposts had been established in order to keep all persons out of danger, as the effects of the instantaneous explosion of 12,000 lbs. of powder were a source of some anxiety to those concerned, and the concussion of the earth that would be made, and the distance to which the stones would be thrown, were both elements of uncertainty to those in charge, and were to those indirectly concerned, matters of the wildest conjecture.

At 7.49 P.M., when it was morally certain that no person was sufficiently near to be injured, Mr. Frank Firmstone gave the word of command, and the writer closed the circuit by pressing on a telegraph key. Mr. Firmstone describes the effect as being two slight but distinct shocks, and a third indistinct one, followed by the dull crunching sound of the falling rocks.

Mr. Blake felt two shocks only, followed by the sound of the falling rocks, and the writer, who, in firing the blast, stood on a scantling which formed the sill of the battery-house, and not on the ground, as did the others, felt neither shock nor jar, heard no sound, and was conscious of no effect whatever, until the sound of the falling rocks reached him.

The rocks fell for about a minute continuously, and for the next half hour spasmodically. The following morning showed a very large mass of stone, of all sizes, but fairly broken up, lying at the foot of the cliff; some of the separate stones contained as much as 600 tons, being 18 feet x 16 feet, x 28 feet in dimensions. In the old quarry a mass of over a thousand tons was shaken down by the blast.

A calculation of the amount thrown down would have been made, but so many elements of uncertainty, such as the distance back of the powder-chamber blown out, the amount below shattered, and the exact sizes of the craters would have to have been taken into consideration that the results would be exceedingly inexact.

From the beginning of the work, a careful account of the cost of labor, material, and other expenses had been kept, which with some other items of interest is here given.

STATEMENT OF THE COST OF BLAST.

1878.

MAIN TUNNEL ALONG SEAM IN ROCK.

Feb. 16,	Drill steel, 45½ lbs. @ 15 c.,	$6 83
" "	Tools, sledge, wedges, powder can, lamps, and oil can,	6 37
" 18,	2 days @ $1.10 making open cut,	2 20
Mar. 1,	Driving main tunnel, 25 ft. @ $1 08⅓ per ft.,	27 08
" 16,	1¼ days @ $1.10, cutting timber for gangway,	1 37
" 19,	Stove-pipe for ventilation, 168 ft. @ 10 c,	16 80
" 26,	Driving main tunnel, 212 ft. 6 in. @ 91⅝ c per ft.,	194 79
" "	Supplies, 1 keg powder, $2.90. 100 ft. fuse, 31 c. 5 balls lamp-wick @ 5 c., 25 c. 5 gal. lard oil @ 63 c., $3 15,	6 61
	Cost of main gangway, 237 ft. 6 in. long,	$262 05

Cost per lineal foot, $1.13. Begun Feb. 19th; finished March 26th, in 56 shifts.

Average progress per shift, 4 ft 3 in.

NO. 1 CROSS-CUT TUNNEL.

Mar. 26,	Drill steel, 63 lbs. @ 15 c.,	$9 45
" 27,	Driving tunnel, 5 feet @ $2 per foot,	10 00
" 31,	" " 25 " " $3 " "	75 00
April 20,	" " 5 " " $8 " "	40 00
" 26,	" " 5½ " " $5 " "	27 50
	Supplies, 5 kegs powder, $14.50. 800 ft. fuse, $2 52. 3 balls lamp-wick, 15 c. 8½ gallons lard oil, $5.36,	22 53
	Cost of No. 1 cross-cut tunnel, 40 ft. 6 in. long,	$184 48

Cost per lineal foot, $4.55. Begun March 26th; finished May 2d, in 58 shifts.

Average progress per shift, 8 inches.

NO. 1 SHAFT.

May 3,	Drill steel, 15¾ lbs. @ 15 c.,	$2 40
" "	Sinking shaft, 8 feet @ $5.50,	44 00
" 10,	" " 10 " " $6.00,	60 00
	Supplies, 3 kegs powder, $8.70. 700 ft. fuse, $2.20. 7 balls lamp-wick, 35 c. 4 gals. lard oil, $2.52,	13 77
	Cost of No. 1 shaft, 18 ft. deep,	$120 17

Cost per lineal foot, $6.68. Begun May 2d; finished May 25th, in 36 shifts.

Average progress per shift, 6 in.

NO. 1 POWDER-CHAMBER.

May 25,	Size, 5 ft. 3 in. square by 6 ft. long. Paid for opening, .	$45 00
	Supplies, 1 keg powder, $2.90. 200 ft. fuse, 63 c. 1 gal. oil, 63 c., ...	4 16
	Cost of No. 1 powder-chamber, ...	$49 16
	Begun May 25th; finished June 11th, in 11 shifts.	

NO. 4 SHAFT.

April 15,	Cleaning and squaring up gangway, ...	$2 00
" "	Drill steel, 31 lbs. @ 15 c., ...	4 65
" 16,	Sinking shaft, 8 feet @ $1.33⅓ per foot, ...	10 67
" 19,	Drill steel, 34½ lbs. @ 15 c., ...	5 18
" "	Sinking shaft, 8 feet @ $3 per foot, ...	24 00
" "	Two picks and handles, ...	2 00
" "	Drill steel, 15¾ lbs. @ 15 c., ...	2 41
" 30,	Sinking shaft, 8 feet at $6 per foot, ...	48 00
	Supplies, 2 kegs powder, $5.80. Giant-powder, $24. 100 ft. fuse, 200 ft. waterproof fuse, $1 31. 5 balls lamp-wick, 35 c. 2½ gallons oil, $1.58, ...	33 04
	Cost of No 4 shaft, 24 feet deep, ...	$131 95
	Cost per lineal foot, $5.50. Begun April 15th; finished May 10th, in 42 shifts.	
	Average progress per shift, 7 inches.	

NO. 4 POWDER-CHAMBER.

May 10,	Size, 5 ft. square by 6 feet long. Paid for opening, . .	$48 00
	Supplies, giant-powder, $12 89. 200 ft. waterproof fuse, $1. 2 balls lamp-wick, 10 c. ½ gal. oil, 32 c, . . .	14 31
	Cost of No. 4 powder-chamber, ...	$62 31
	Begun May 10th; finished May 20th, in 14 shifts.	

NO. 2 CROSS-CUT TUNNEL.

May 21,	Drill steel, 32 lbs. @ 15 c., ...	$4 80
	Driving tunnel, 9 feet @ $4 per foot, ...	36 00
" 28,	" " 16 ft. 6 in. @ $5 per foot, ...	82 50
	Supplies, giant-powder and caps, $29 66. 400 ft. fuse, $1 26. 4 balls lamp-wick, 20 c. 3 gals. oil, $1.89, .	33 01
	Cost of No. 2 cross-cut tunnel, 25 feet long, ...	$156 31
	Cost per lineal foot, $6.13. Begun May 21st; finished June 15th, in 45 shifts; 36 were 10-hour shifts, and 9 were 8-hour shifts.	
	Average progress per shift, 7 inches.	

NO. 2 SHAFT.

June 15,	Sinking shaft, 19 ft. 6 in. @ $5.50 per foot, ...	$107 25
	Supplies, giant-powder and caps, $30.25. 150 feet fuse, 47 c. 2 balls lamp-wick, 10 c. 2½ gals. oil, $1 57, .	32 39
	Cost of No. 2 shaft, 19 ft. 6 in. deep, ...	$139 64
	Cost per lineal foot, $7.16. Begun June 15; finished June 20th, in 30 8-hour shifts.	
	Average progress per shift, 8 inches.	

NO. 2 POWDER-CHAMBER.

June 26,	Size, 5 ft. 3 in. square by 6 feet long. Paid for opening, .	$30 00
	Supplies, giant-powder and caps, $5. Fuse, 15 c. Lamp-wick, 10 c. Oil, 63 c., ...	5 88
	Cost of No 2 powder-chamber, ...	$35 88

Begun June 26th; finished July 2d, in 15 8-hour shifts.

NO. 3 CROSS-CUT TUNNEL.

June 1,	Driving tunnel, 10 ft. @ $4 per foot, ...	$40 00
	Supplies, giant-powder, $10. Fuse, 15 c., ...	10 15
	Cost of No. 3 cross-cut tunnel, 10 feet long, ...	$50 15

Cost per lineal foot, $5 01. Begun June 1st; finished June 7th, in 10 shifts.

Average progress, 1 foot per shift.

NO. 3 SHAFT.

June 7,	Sinking shaft, 8 feet @ $4 per foot, ...	$32 00
	" " 13 ft. 3 in. @ $6 per foot, . : . .	79 50
	Supplies, giant-powder, $21. 350 ft. fuse, $1.10. 7 balls lamp-wick, 35 c. 3 gallons oil, $1.89, ...	24 34
	Cost of sinking No. 3 shaft, 21 ft. 3 in. deep, ...	$135 84

Cost per lineal foot, $6.39. Begun June 7th; finished July 1st, in 35 shifts.

Average progress per shift, 7 inches.

NO. 3 POWDER-CHAMBER.

July 2,	Size of chamber, 5 ft. 3 in. by 6 ft. long. Cost of opening,	$48 00
	Supplies, giant-powder, $6 75. Lamp-wick, 10 c. 2 gals. oil, $1.25, ...	8 11
	Cost of No. 3 powder-chamber, ...	$56 11

Begun July 2d; finished July 12th, in 15 shifts.

DRILLING ABSORBING WELL IN NO. 4 SHAFT.

July 1,	11½ days @ $1.10, ...	$12 65
	Supplies, giant-powder, $2.55. Fuse, 20 c. Lamp-wick, 15 c. Oil, 95.	3 85
	Cost of drilling well and explosive, ...	$16 50

Depth 14 feet. Cost per lineal foot, $1.18.

GENERAL MINING EXPENSES.

Feb. 14,	Travelling expenses, ...	$1 73
April 26,	Stove-pipe and elbow, ...	5 40
" 30,	Blowing fan for ventilation, ...	12 00
May 31,	" " "	26 00
June 30,	" " "	45 00

July 10,	Paving No. 4 shaft with cobble stones,		$1 40
11,	Covering floor of tunnel with loam,		7 20
16,	Cutting hitches, 6 days @ $1,		6 00
23,	Drilling holes for wire supports, 10 days @ $1,		10 00
31,	Blowing fan for ventilation,		8 00
			$122 73

POWDER AND TAMPING ACCOUNT.

Aug. 3,	Hemlock lumber for powder-boxes, 500 feet @ $10 per M,		$5 00
"	Making powder-boxes,		8 00
4,	Erecting powder-boxes,		8 00
5,	Making ladders,		2 50
7,	Putting sand around powder-boxes, 1 day @ $2, 2 @ 1.25, 28 @ 1.10, 16 @ 1.00, 1 @ .60,		51 90
8,	Powder-troughs, hoops, squares for powder-barrels, and handles,		6 00
"	Digging and screening sand, 2 men @ $1.10, 2 @ 1.00,		4 20
9	Erecting powder-troughs, 1 man @ $2, 7 @ 1.10,		9 70
"	Screening sand, 4 men @ $1,		4 00
10,	Placing powder in boxes, 1 day @ $2, 1 @ 1.25, 21 @ 1.10, 10¾ @ 1.00, 10 @ .90,		46 10
"	No. 1 took 35 minutes to fill it, and cost,	$ 6.72	
"	No. 2 " 45 " " "	8.65	
"	No. 3 " 50 " " "	9.60	
"	No. 4 " 1 hr. 50 min. " "	21.13	
"	Taking out troughs,		4 00
11,	Watching tunnel, 4 men @ $1 10,		4 40
12,	Filling shaft with sand, 1 day @ $2, 2 @ 1.25, 38¾ @ 1.10, 22½ @ 1.00, 10 @ .90, 1¼ @ .60,		79 37
13,	Filling shafts with sand, 1 day @ $2, 3 @ 1.25, 29 @ 1.10. 14 @ 1.00, 10 @ .90, 1 @ 60 (Loam tamping, $15.83, divided among 1, 2, and 4 gangways, $5 28 each),		61 25
	No. 1 shaft, distant 95 feet from mouth, took 1278 kegs of sand, was filled in 3 hours and 50 minutes, and cost $26.47 to fill.		
	No. 2 shaft, distant 166 feet from mouth, took 1319 kegs of sand, was filled in 6 hours 30 minutes, and cost $31.64 to fill.		
	No. 3 shaft, distant 194 feet from mouth, took 971 kegs of sand, was filled in 3 hours 15 minutes, and cost $21.84 to fill.		
	No. 4 shaft, distant 233 feet from mouth, took 1024 kegs of sand, was filled in 5 hours 10 minutes, and cost $31.64 to fill.		
Aug. 14,	Loam tamping, 1 day @ $2, 1¼ @ 1.90, 2 @ 1.25, 29¼ @ 1.10, 22 @ 1.00, 2½ @ .90, 2 @ .60,		64 49
"	Loam tamping and taking up track, 1 day @ $2, 3 @ 1.25, 27¼ @ 1.10, 15½ @ 1.00, 1 @ .60,		52 82
"	Erecting buttresses, 3 carpenters ½ day,		3 25
"	Powder, 12,000 pounds @ 13.6 cents per pound,		1632 00
			$2053 73

ELECTRICAL ACCOUNT.

Aug. 5,	Fuses, 100 @ 4 cents,	$4 00
10,	Travelling expenses,	12 59
"	Rheostat, galvanometer, wire, battery zincs, solution, and connections,	212 09
		$228 68

RELAYING TRACK.

Aug. 16,	4 days @ $1.10, 5 days @ 1.00,	$9 40
17,	4 days @ $1.10, 3 days @ 1.00,	7 40
19,	1 day @ $1.10,	1 10
		$17 90

RECAPITULATION.

Main tunnel along seam in rock, 237 feet 6 inches long,	$262 05	
No. 1, cross-cut tunnel, 40 feet 6 inches long,	184 48	
No. 1, shaft, 18 feet deep,	120 17	
No. 1, powder-chamber,	49 16	
No. 4, shaft, 24 feet deep,	132 95	
No. 4, powder-chamber,	62 31	
No. 2, cross-cut tunnel, 25 feet 6 inches long,	156 31	
No. 2, shaft, 19 feet 6 inches deep,	139 64	
No. 2, powder-chamber,	35 88	
No. 3, cross-cut tunnel, 10 feet long,	50 15	
No. 3, shaft, 21 feet 3 inches deep,	135 84	
No. 3, powder-chamber,	56 11	
Drilling absorbing well in No. 4 shaft, 14 feet deep,	16 50	
General mining expenses,	122 73	
		1524 28
Powder and tamping account,	$2053 88	
Electrical account,	228 68	
Relaying track,	17 90	
Total powder account,		2300 36
Gross total,		3824 64
By 165 pounds of steel @ 15 cents,	$25 00	
" Tools, less deterioration,	7 00	
" Stove-pipe 150 feet @ 10 cents,	15 00	
" Ladder,	2 00	
" Rheostat, galvanometer, reclaimed wire, zincs, solution, and connections,	207 00	
" 10 Fuses @ 4 cents,	40	
		256 40
Net cost of blast,		$3568 24

The following table shows the relation in cost of the different items of mining, including both the cost per lineal foot of the work and the percentage.

The remarks made upon giant-powder in another portion of this paper, are sustained by the high percentage of the explosive account when it is used, and the reverse when blasting powder is employed.

The slight discrepancy in the cost per foot in the two tables is caused by the amount of steel used up, which is itemized in the latter and charged to a single account in the former.

PERCENTAGE OF COST ACCOUNT IN MINING.

	Cost per foot.	Percentage.
MAIN TUNNEL.		
Making open cut, labor,	$ 01	.009
Cutting timber for gangway, labor,	01	.005
Stove-pipe for ventilation,	07	.068
Driving main tunnel, labor,	94	.891
Powder,	01	.012
Fuse,		.001
Lamp-wick,		.001
Lard oil,	01	.013
	$1 05	1 000
NO. 1 CROSS-CUT TUNNEL.		
Drill steel used,	$ 06	.013
Driving No. 1 tunnel, labor,	3 77	.860
Powder,	36	.082
Fuse,	06	.014
Lamp-wick,		.001
Lard oil,	13	.030
	$4 38	1.000
NO. 1 SHAFT.		
Drill steel used,	$ 05	.008
Sinking No. 1 shaft, labor,	5 78	.876
Powder,	48	.073
Fuse,	13	.019
Lamp-wick,	02	.003
Lamp oil,	14	.021
	$6 60	1.000
NO 1. POWDER-CHAMBER.		
Drill steel used,	$ 06	.007
Opening No. 1 chamber, labor,	7 50	.909
Powder,	48	.058
Fuse,	10	.013
Lard oil,	11	.013
	$8 25	1.000

NO. 4 SHAFT.

	Cost per foot.	Percentage.
Drill steel used,	$ 05	.011
Cleaning and squaring up gangway,	08	.017
Sinking No. 4 shaft, labor,	3 46	.695
Powder,	25	.049
Giant powder and caps,	1 00	.201
Fuse,	05	.011
Lamp-wick,	01	.003
Lard oil,	06	013
	$4 96	1.000

NO 4, POWDER-CHAMBER.

Drill steel used,	$ 06	.005
Opening No. 4 powder-chamber, labor,	8 00	.768
Giant-powder and caps,	2 16	.207
Waterproof fuse,	16	.014
Lamp-wick.	01	.001
Lard oil,	05	.005
	$10 44	1.000

NO. 2, CROSS-CUT TUNNEL.

Drill steel used,	$ 05	.009
Driving No. 2 tunnel, labor,	4 66	.776
Giant powder and caps,	1 16	.194
Fuse,	05	.008
Lamp-wick,	01	.001
Lard oil,	07	.012
	$6 00	1.000

NO. 2 SHAFT.

Drill steel used,	$ 06	.008
Sinking No. 2 shaft, labor,	5 50	.762
Giant powder and caps,	1 55	.215
Fuse,	02	.003
Lamp-wick,	01	.001
Lard oil,	08	.011
	$7 22	1.000

NO. 2 POWDER-CHAMBER.

Drill steel used,	$ 05	.009
Opening No. 2 powder-chamber, labor,	5 00	.828
Giant powder and caps,	84	.139
Fuse,	03	.004
Lamp-wick,	02	.003
Lard oil,	10	.017
	$6 04	1.000

NO. 3. CROSS-CUT TUNNEL.

	Cost per foot.	Percentage.
Drill steel used,	$ 06	.012
Driving No. 3 cross-cut tunnel, labor,	4 00	.788
Giant powder and caps,	1 00	.197
Fuse,	01	.003
	$5 07	1.000

NO. 3 SHAFT.

Drill steel used,	$ 05	.008
Sinking No. 3 shaft, labor,	520	.814
Giant powder and caps,	98	.153
Fuse,	05	.008
Lamp-wick,	02	.003
Lard oil,	09	.014
	$6 39	1.000

NO 3 POWDER-CHAMBER.

Drill steel used,	$ 05	.006
Opening No. 3 powder-chamber, labor,	8 00	.850
Giant powder and caps,	1 12	.119
Lamp-wick,	02	.002
Lard oil,	22	.023
	$9 41	1.000

DRILLING ABSORBING WELL IN NO. 4 SHAFT.

Drilling well, labor,	$ 91	.766
Giant powder,	18	.155
Fuse,	01	.012
Lamp-wick,	01	.009
Lard oil,	07	.058
	$1 18	1.000

RESULTS.

Until the exact amount of rock loosened is known, it is difficult to speak with precision in regard to the economic success of the blast. The services of 10 men, paid at the rate of $1.00 per day, were dispensed with shortly after it was fired. This, allowing 275 working days to the year, would be a saving at once of $2750, supposing the rock thrown down to hold out so long, which at present seems probable.

Other items of saving are known, but their discussion will not be entered into until it is possible to ascertain exactly the amount of stone thrown down by the blast.

No. 4 powder-chamber did very little work in throwing down rock.

No. 1 powder-chamber was located too far from the mouth of the gangway, so that a buttress of rock was formed which kept a large mass of stone from falling which would otherwise have reached the ground.

About 2 months after the firing of the blast, a portion of the main gangway was cleaned out and timbered for possible future use. There was but little to be done, except light timbering as far as No. 2 cross-cut, but beyond that it was entirely closed in. No. 1 cross-cut was cleaned out by having the tamping removed, and it was found that the force of the powder had nowhere in the gangway acted upon the tamping, but that the rock forming the sides of the gangway had crushed together to the extent of about a foot.

The wire which was laid along the floor, was found in the parts near and in the shaft, to be broken into pieces of about a foot in length. This singular circumstance may be accounted for by the hypothesis that the first result of the explosion was a succession of sharp, undulatory motions of the rock which acted upon the taut wire with the described effect.

The rock about the end of No. 1 cross-cut was considerably shattered, and the shaft was only to be distinguished by the column of tamping. At the end of the cross-cut, which was originally tightly tamped with loam, an empty space was found on the reopening of the gangway. This was presumably caused by the blowing out of the powder-chamber and lower portion of the shaft, and the sinking of the column of sand, carrying with it the loam tamping of the gangway.

The time occupied from the inception of the work to its completion by the closing of the electric circuit was six months.

THE MANUFACTURE OF SODA BY THE AMMONIA PROCESS.

BY OSWALD J. HEINRICH, MINING ENGINEER, DRIFTON, LUZERNE COUNTY, PA.

THE serious objections to the Leblanc soda process may be enumerated as follows: 1st. The total loss of sulphur employed, equal to about one-third of soda produced. Various processes have been proposed to regain this sulphur, but none have come into practical operation on a large scale. 2d. The great bulk of waste products, equal to nearly twice the amount of soda, which forms a serious annoyance about the works. 3d. The complicated series of operations necessary. 4th. The large amount of labor and fuel required. 5th. The production of noxious gases which necessitate extensive condensing arrangements. 6th. The large capital necessary for the erection and maintenance of the plant. These objections have long been recognized, and many attempts have been made during the last thirty or forty years, to discover and introduce a cheaper and more direct method for the manufacture of this important staple. The reaction between bicarbonate of ammonia and common salt, whereby bicarbonate of soda, insoluble in a solution of chloride of ammonium, is precipitated, has been closely studied for the last twenty-four years, on account of its simplicity and the purity of the product it affords.

This ammonia-soda process is now so far developed, that while it is not in position to entirely replace the Leblanc process, yet it can successfully compete with it where the conditions and localities are favorable. The advantages of the ammonia-soda process may be summed up as follows:

1st. It is independent of the essential agent in the Leblanc process, viz., sulphuric acid, and hence can be established in those localities where salt is abundant, and yet where sulphuric acid could not be made, or where it could not be transported profitably.

2d. The ammonia-soda process being a "wet" process, the evaporation of salt brines to obtain the solid salt required in the Leblanc process, is rendered unnecessary.

3d. The *direct* conversion of chloride of sodium into bicarbonate of soda, which can readily be converted into the maketable monocar-

bonate of a degree of purity of 98 to 100 per cent., free from sulphur compounds and from the contaminating salts of the mother liquor.

4th. The simplicity of the apparatus required.

5th. Economy of fuel, and also of labor which need not be highly skilled.

6th. The absence of noxious gases and of objectionable waste products.

7th. The smaller investment required in plant, which is only one-tenth of that required in the Leblanc process for the same production.

8th. As a result of the above enumerated advantages, the cost of production in favorable localities will be about 66 per cent. of the market price.

The disadvantages of the process as advanced by its opponents may be said to be:

1st. Great loss of ammonia, and therefore, that it is only available in places where ammonia is manufactured.

2d. The loss of chlorine which is regained as chloride of calcium, a material of but little use or value, if not worthless.

The following opinions of chemists of high reputation may be cited as important testimony on the subject.

Prof. A. W. Hoffmann (Official Catalogue, III Group, Vienna Exposition, 1873), says:

"At all events the ammonia-soda process is the only one which will prove a serious competitor to the now nearly exclusively used Leblanc process."

Prof. Roscoe, F.R.S., in a lecture upon technical chemistry, at South Kensington, in 1877, concluded as follows:

"Out of the very many proposals which have been made to replace Leblanc's original reactions by others, only one has been practically successful. This is theoretically a very simple and beatiful one, and *in skilful hands* has been found capable of being worked on a manufacturing scale. . . . . Nothing can surely be simpler or more beautiful than this—no evolution of noxious fumes, no waste of sulphur; by a simple decomposition we at once obtain what we want. The precipitate consists of bicarbonate of soda, insoluble in the ammoniacal brine, whilst sal ammoniac remains in the liquid, and from it the ammonia can be recovered by heating the liquor with lime. Simple and beautiful as this process appears to be, the difficulties which surround it are extremely great. Although there is doubtless a future for this process, it is very unlikely

that it will interfere with the manufacture of soda by the old plan. One reason against the process is, that in the old process the manufacturer not only makes use of the sodium of the common salt, but likewise of the chlorine; indeed, in many alkali works the manufacture of bleaching powder is the main, that of alkali the subsidiary end. Until, therefore, we can find a cheap mode of extracting the chlorine from the chloride of calcium this method will hardly be much used, unless, indeed, Mr. Weldon's proposal for employing magnesia instead of lime, and for decomposing the chloride of magnesium by steam, and thus getting hydrochloric acid, turns out to be practically successful. The advantage of the process is, that the product obtained is practically pure, and the soda ash thus prepared tests up to 58.5 per cent. of alkali (against 48 per cent. by the Leblanc process)."

Fair and candid as this opinion is, we must not overlook the fact that Prof. Roscoe lives in a locality which is extremely favorable for the Leblanc process, and where this manufacture is most extensively developed.

We will add in regard to the two principal objections stated above, the following remarks:

1st. With regard to the loss of ammonia, experience has shown that in properly constructed apparatus with air-tight vessels and joints, the loss by leakage or otherwise is insignificant in comparison with the important advantages which the process presents, and is, moreover, fully taken into account in the estimates of the cost of manufacture. Theoretically there is no ammonia lost whatever, if the process is carried out with care by a skilful manager.

2d. The chloride of calcium is not necessarily lost. It can be used as an absorbent of moisture in drying gases, and in the rectification of spirits, in the preparation of tartaric acid, as a fertilizer, in the manufacture of glass, and also in the preparation of chloride of barium, even though its conversion into bleaching powder has not yet been accomplished. In some localities it can be used in the production of firebrick. Experiments are still in progress to utilize profitably this product, but even if it were entirely lost, it would not effect the cost of the soda so as to prevent the process being introduced successfully, as various establishments now working this process abundantly prove. This will appear from the estimates given below.

The principal well-established works now using the process, are those of Messrs. Brunner, Mond & Co., at Northwich; E. Solvay,

at Couillet, Belgium, and at Varangeville—Dombasle, France; Schlœsing & Rolland, at Pateaux, near Paris; Honigmann, at Aachen & Ludwigshafen, Germany, and Gerstenhöfer, at Nagy-Bosco, Hungary.

The works at Couillet produce 4000 to 5000 tons annually. There are also a number of new works projected, particularly in Germany. The sites selected for these works show that the fact is becoming appreciated, that the localities most favorable for this process are the brine wells and the salt mines. This subject deserves consideration in this country, for we have here a number of localities fully as favorable, if not more favorable than in Germany, where the process would prove a perfect success, and afford a good return to the capitalist.

The cost of establishing such a manufacture must to some extent depend on the locality and the capacity of the works, and no general statement of cost could be well made without particular investigation. Nevertheless, it is certain that works of the least capacity, to reduce the general expenditures to an acceptable minimum, could be built at from $60,000 to $80,000, capable of a daily production of 7500 kg. = 16,500 pounds. Taking this as a unit of comparison with the old Leblanc plant, the cost of production, based upon actual working results taken from the best works of the kind in Europe, reduced to prices of labor and material ruling this country, will be:

	At the cities near the coast.	At brine centres.
Materials, 8½ tons of salt @ $6,	$51 00	$20 00, or more according to locality.
7½ tons of limestone @ $2 50,	18 75	13 00, or less.
8 tons of coke and coal, @ $2,	16 00	8 50, or more.
Labor and salaries, per day,	79 00	79 00, or less.
Packing 82½ barrels (200lb), @ 50 cents, . . .	41 25	33 00
Repairs and materials, etc., .	23 00	23 00
Incidental expenses, . .	15 00	15 00
Interest upon $80,000 capital, invested at 10 per cent., .	26 66	26 66
Total per day,	$270 66	$218 16

The production per day will be 16,500 pounds of dry monocarbonate of soda of 98 per cent. Considering the chloride of calcium to be a total loss, although it may be estimated at least at the rate of common lime, the cost of manufacture, allowing ten per cent. interest in the estimate, will be respectively 1.64 cents or 1.32

cents per pound. At the rate of 2 cents for commercial soda of 48 per cent. alkali, this soda of 58.5 per cent. alkali would be worth at least 2½ cents, leaving a net profit of .86 cent, and 1.18 cents per pound, or \$141.90 and \$194.70 per day net profit.

It has been asserted that the loss of ammonia by this process is at least 5 per cent., an amount which by careful work and improved apparatus is unquestionably an exaggeration. But suppose such a loss should exist; at the rate of \$100 per ton of sulphate of ammonia as a source of ammonia to supply the deficiency, the daily loss in money of 4 per cent. extra loss of ammonia would be \$82.70, or .38 cent per pound soda manufactured, increasing the cost of manufacture respectively to 2.02 cents and 1.70 cents per pound. But the loss is in reality very insignificant, and should never at the outside exceed 1 per cent. (at least where the highly perfected apparatus of Mr. Gerstenhöfer, of Saxony, is used), for which allowance has been made in the above statement.

According to E. Rolland in France, the manufacture of soda by this process, at the rate of 9000 kg. = 19,800 pounds per day would be .88 cent per pound, and at a daily production of 10,000 kg. = 22,000 pounds even as low as .8 cent. In Germany, the cost according to Gerstenhöfer, will be .98 cent per pound @ 7500 kg. = 16,500 pounds per day, if rock salt is used, and according to M. Honigmann, in Aachen, the cost will only be .75 cent, while the wholesale market price of this article is from 1.50 to 2.35 cents per pound.

The ammonia in this process is theoretically entirely regained, and acts only as a carrier in the process. No loss of ammonia in the reaction can, or should exist, the only loss being from leakage or waste not properly used over again. In this respect the apparatus of Gerstenhöfer deserves the highest praise for its ingenuity of arrangements. In converting the bicarbonate into the monocarbonate an equivalent of carbonic gas is regained, but the direct sale of this bicarbonate, where absolute purity is not required, would prove equally profitable. The objection has been made against some of the soda manufactured by this process, that it has the peculiar smell of ammonia. Bicarbonate manufactured directly by this process was tested by Prof. P. Frazer, Jr., of Philadelphia, by the Nessler test, and yielded only .15 per cent. of ammonia and had no peculiar smell. To have it quite pure, the conversion into monocarbonate first and the reconversion into bicarbonate afterwards, is preferable. This product is used in the preparation of

caustic soda of high purity, which cannot be obtained by any other process. This is particularly sought for by manufacturers of the aniline colors, paraffin manufacturers, and also by the manufacturers of the finer toilet soaps, and is, therefore, of importance to this country.

A more favorable exhibit can be made if the localities selected are favorable, and if the production is large. For example, the cost for brine, nearly 19 per cent. strength, in the Syracuse district, for one ton of salt will be about $2.20, and allowing for the expense of concentration, it would not cost over $3. In the Kanawha region of West Virginia, although the brine is only 8 per cent., it would cost only $2.35 on account of the cheap fuel. In southwest Virginia, at the Holston salt region, the wells furnish an abundance of saturated pure brine, which would even have to be diluted to answer the purpose, and which could be obtained at still lower rates. The success of the process depending largely upon the purity of the materials employed, the locality, and facilities of transportation, it is of great importance that a thorough investigation of all these items should precede the establishment of a plant.

The opponents of the process point to the secrecy with which the works are surrounded, as evidence that it is not yet commercially successful, but there are good reasons for this secrecy. The chemical reactions cannot properly be regarded as subjects of patents. They were known more than half a century ago. We find in the *Chemisches Centralblatt*, for 1874, that a well-known chemist, Aug. Vogel, found among his father's papers of 1822, under the head of "Unfinished Experiments," the following memorandum: "Concentrated solution of table salt is strongly precipitated by carbonate of ammonia. It would be worth while to ascertain if the same precipitate occurs in a solution of chemically pure salt, or if the precipitate is only carbonate of lime resulting from impurities in the salt." There are several patents in existence, some even in this country in which this reaction enters into the claim. Of these may be mentioned E. Solvay's process patented in France, Belgium, and this country, and James Young's, patented also in England and the United States. The patent of H. de Grousillier's and G. Siemen's, differs in some respect from the preceding. The apparatus of the first two mentioned differ considerably, but both are objectionable on account of the loss of ammonia. Grousillier uses absolute alcohol to remove the ammonia without dissolving the carbonate of soda—an interesting process, but too costly. Gersten-

höfer and Honigmann, the two most successful operators in Germany, used an apparatus widely different from the others mentioned, whereby the loss of ammonia is prevented.

The cause which operates most strongly against the introduction of the ammonia-soda process, is found in the immense amount of capital invested in the Leblanc process, particularly in England, where the manufacture is combined with that of acid and bleaching-powder, the latter being the principal source of profit. The manufacture of soda alone would not pay expenses, and the acid produced has also a limited sale, and much of it runs to waste. Should these works be remodeled to make soda by the new process a great loss would be incurred, which must be postponed as long as possible.

But these arguments do not apply to this country. We still import a very large amount of soda ash, and also manufacture to some extent soda from cryolite, a material imported from Greenland, where the deposits are nearly exhausted. What, then, prevents us from erecting in our own interests, works of the newer and cheaper system to produce the soda we now import, leaving to the older process the production of soda as an incidental product of the acid and bleaching powder manufacture? According to the official government statistics, the importation into this country of soda products (the amount which remained for consumption), during the depressed market of the twelve months ending June, 1877, was as follows:

Of bicarbonate of soda, .	4,299,906 pounds,	@ 2.5 cts., .	$107,069
Of soda ash and salt soda, .	218,316,863 "	@ 1.57 cts. .	3,440,751
Of caustic soda, . . .	35,066,818 "	@ 3 2 cts. .	1,123,946
Or a total of . .	115,037.3 tons at	. . . .	$4,671,766

This shows the sum of money we are sending out annually to foreign countries, for a product for which the raw materials are in abundance in this country, and which can be equally successfully manufactured at home. We also see that with this large consumption of soda there is little danger of overstocking the market, even should several large establishments be engaged in its manufacture.

To illustrate again, the advantages of the ammonia process, the following statements, obtained from the statistics of the alkali trade of the United Kingdom of Great Britain, given by the Alkali Association for England, in 1876, for the Leblanc process, and for the ammonia-soda process from personal investigation of actual working results in Germany, are given in Table No. 1.

In Table No. 2, is a statement by Prof. Roscoe, in the lecture

already referred to, from the estimate of Mr. Maetear, of the amount of material required in an alkali works, working 100 tons of pyrites, and the products produced therefrom:

TABLE I.

	Ammonia process in the United States requires per ton of carbonate of soda.	Leblanc process in England requires per ton of carbonate of soda.
	Tons.	Tons.
Common salt,	1.16	1.068
Limestone (and lime,)	1.02	0.861
Coke and coal,	0.98	2.236
Pyrites,	——	0.445
Saltpetre,	——	0.014
Manganese,	——	0 021
Total materials,	3.16	4.645
Labor,	0.021 man,	0.026 man.
Wages,	$10.73	$8.06
Capital employed,	$3.668	$40.177
Repairs,	$3.12	$2.347
Cost of product,	$29.36	$37.30
Export value,	——	$39.56
Market value,	$56.00	——

TABLE II.

Raw materials required and products formed in the Leblanc process per 100 tons of pyrites:

Raw materials.		Products.	
100.00 tons	pyrites,	70.00 tons	burnt ore.
1.88 "	saltpetre,	65.45 "	bleaching powder.
160.35 "	common salt,	134.05 "	soda ash of 48 pr. ct. alkali or 90.63 tons soda of 60 per ct. or 242 8 tons soda crystals.
47.45 "	manganese,		
36.81 "	lime,		
123.46 "	limestone,		
67.00 "	coal,	42.75 "	manganese recovered.
536.95		312.25	

If the pyrites are too poor in copper to render its extraction profitable, the "burnt ore" is almost valueless. It will be noticed in this second table that the sulphur, a substance less abundant and more valuable than chlorine, is totally lost.

It thus appears that in the Leblanc process over four tons of material are employed for one ton of product, and fully two tons of this

material must be reckoned in the waste, even when the "burnt ore" and hydrochloric acid can be utilized, which is not always the case. The soda ash produced, moreover, contains but 48 per cent. of alkali.

A corresponding statement for the ammonia-soda process is found in the following table:

TABLE III.

Raw materials.	Products.
14 tons common salt,	5.44 tons salt regained.
7½ " limestone	8.25 " chloride of calcium and caustic lime.
8 " coal, . .	7.36 " dry monocarbonate of soda of 58½ pr. ct. alkali.
29½	21.05 tons.

Thus, while about four tons of raw materials are used per ton of product sought, there are but 1.12 tons of waste if all the caustic lime and chloride of calcium are so considered, which is not necessarily the case. These substances are, moreover, abundant in nature, and can be obtained at low cost. Further, the product contains 58½ per cent. of alkali.

The depressed state of our industries suggests the desirability of multiplying our manufactures that they may profitably react on one another, and it would seem to be the part of prudence for capitalists to embark in those enterprises which are free from fanciful or fictitious elements, that a suffering and willing population of laborers may find employment in the development of those resources with which a kind Providence has plentifully supplied us. Progress in the arts compel frequently the change of site and of the principles of manufactures, and those who are the first to recognize and avail themselves of this inevitable change, will be those who will reap the largest profits.

AN IMPROVED UNIVERSAL SUSPENDED HYDRAULIC LIFT.

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SOME time ago the writer needed a cheap, light, and portable hydraulic lift or crane, that would be universal in its application, and that might be suspended from crane-arms, overhead tramways, beams, etc., and serve all the uses of ordinary hydraulic cranes. This was accomplished by the use of a closed cylinder of wrought-iron hydraulic pipe, provided at its top with the ordinary gland and stuffing-box, through the centre of which passed another similar pipe of slightly smaller diameter, forming in effect the ordinary plunger of the regular hydraulic crane.

Over the top of this centre pipe or plunger or piston a disk of iron is placed with two arms projecting several inches from the rim, as may be required. Through these arms long iron rods pass, one on each side, and are fastened in place by nuts screwed to a bearing immediately above and below the disk. These rods also pass through the enlarged head of the lift down through a casting which closes the bottom of the cylinder, which, at the same time, gives them a bearing, and they terminate at a point below the casting equal to the height of the lift of the crane.

The rods at this point pass through a disk similar to the top disk and are similarly secured in place. A common hook arranged with a swivel passes through the centre of the disk, and on this hook the load is suspended.

For a stationary hanging crane the apparatus is suspended by two rods passing through the head of the lift in similar manner to the two already described, at right angles to the first, and of sufficient length to let the first set play between them and rise to the full height which the plunger will allow. The second set of rods terminates in an iron disk similar to the one at the bottom of the first set, and a hook through this in turn suspends the entire apparatus.

In order to adapt the lift to an overhead tramway, single or double, in place of the suspending rods, a projection or offset is arranged on each side of the head of the lift and a journal cast in, allowing the lift to be suspended on pivots arranged as follows:

For a single tramway, with a single wheel or pulley, an axle passes through the centre of the wheel, projecting several inches beyond each side. A connecting bar or rod unites each end of this axle with the ends of the journals below, at a distance equal to the entire lift of the crane, thus allowing swinging motion in the direction of the length of the tramway. When in any case vertical space is more limited, the room available may be economized by the following modification of the lift.

The head of the lift may be moved as close to the bottom of the crane-arm or tramway as will allow it to swing free. The stuffing-box, etc., may be transferred to the *bottom* of the outside cylinder, which is thus left open at the top, and a regular piston head fitted to the top of the plunger, which thus plays in the outside cylinder. The piston is prevented from rising too far by a disk fastened below, as in the first form, which in turn carries the hook as before. For a double tramway, with two wheels on each side, the journals of the lift or crane play in a rectangular frame, in bearings

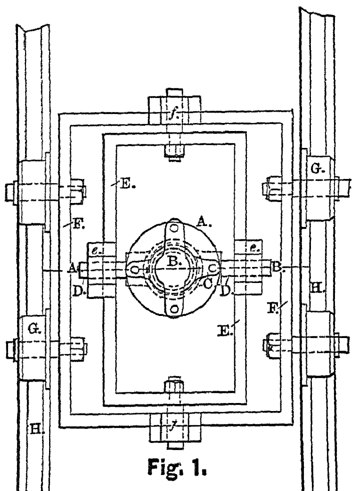


Fig. 1.

of the ordinary kind, motion in every plane being secured by the use of a second rectangular frame in which the first frame hangs suspended at right angles to the axis of the journals suspending the lift proper, thus securing a universal pivot. This second frame is at-

tached to wheels resting on the tramway. In order to save friction these wheels should be of good size, and provided with friction rolls.

The outside cylinder is tapped at the lowest convenient point on its side, and a piece of common iron pipe inserted, and into this short pipe is fastened ordinary rubber hose. The water is forced into the bottom of the cylinder through the rubber hose by the usual means to accomplish such a purpose.

For use on an overhead railway, crane-arms, etc., the rubber hose is carried still higher, and is suspended by a chain attached to a

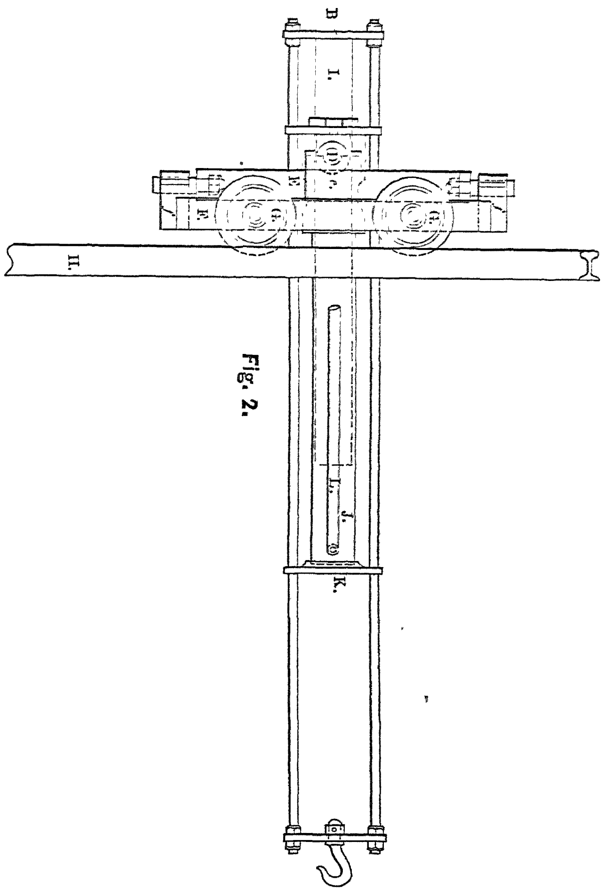


Fig. 2.

single pulley, which in its turn runs along a single suspended horizontal bar, and thus follows the lift in any direction. A common two-way valve, or two globe valves suitably arranged, alternately admits and draws off the water from the described cylinder. The

drawings show the lift adapted for use as an overhead travelling crane. In Fig. 1, B, is the disk surmounting the plunger (shown dotted); A, is the head of the lift; C, is the gland fitting into the stuffing-box; D, is the journal projecting on either side and revolving in the bearings, *e*, fastened to the frame, E. The frame in turn revolves in the bearings, *f*, fastened to the outside frame, F, thus securing a universal motion of the lift. Frame, F, in turn, is supported on four wheels, G, running along the rails, as shown, and revolving on axles passing through F. Fig. 2, shows a side elevation of

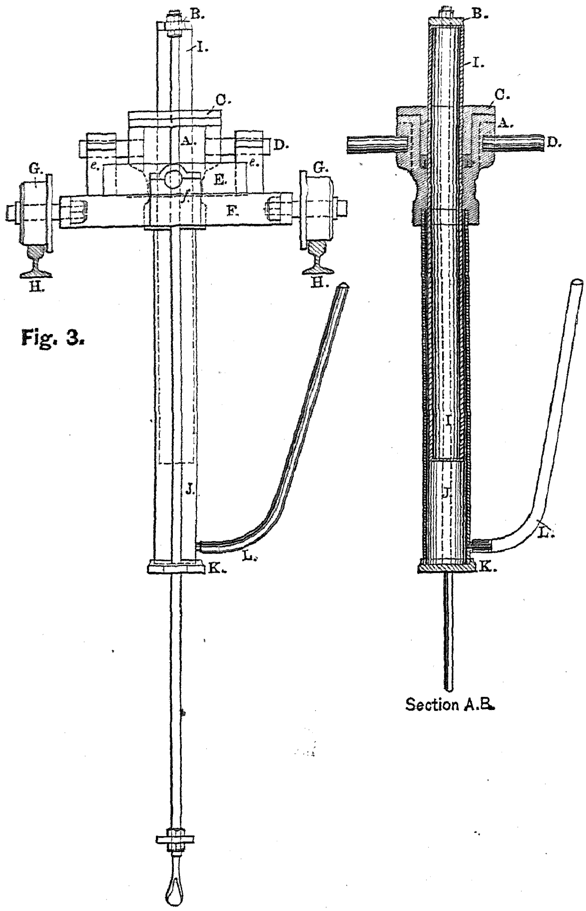


Fig. 3.

the entire apparatus, like letters referring to like parts; J, is the outside cylinder closed by casting, K, through which also pass the weighted rods already mentioned; L, is the supply-pipe passing to source of pressure, and suspended from a pulley overhead (not shown).

This flexible pipe moves with the lift, as before described. Fig. 3, represents a full end view of the lift, like letters indicating like parts. Section, A, B, shows the inside view of the lift proper; J, is the outside cylinder, and I, the plunger. To adapt this arrangement perfectly for a charging crane, a brake (not shown) is arranged, locking the wheels, G, at will to the rails. To withdraw from the furnace, a pair of grab-tongs are swung from the bottom hook, as is usual, and pushed into furnace and clamped to any desired pile or ingot. By raising the piston, the tongs with their load are at once withdrawn, and the load may be transferred by the tramway.

To charge a piece, a pulley is placed beneath the furnace-door, a chain is hooked to the end of a charging peel, then carried over the pulley, and up to the bottom hook of the lift. By clamping the lift, and hoisting the piston as before, the peel can be run into the furnace-door over a movable roll at its mouth. This lift can be hung in a similar way on any crane-arm, single or double, and used as a hydraulic crane at will, at very much less expense of construction and operation than by methods heretofore in use. It may also supplement for light work a common heavy crane, being hung on a light arm from crane posts, as it takes very little water, and thereby saves the steam used in lifting the heavy arms, trucks, etc., of large cranes. In one of the several forms it may be made portable, and serve a great variety of uses in loading merchandise, storing heavy goods, as a stone derrick, etc., as well as many purposes in manufacturing iron and steel.

As hydraulic pipe may be had up to 18 inches in diameter, and rubber hose can be easily procured, capable of standing 300 to 400 pounds to the square inch, this crane, it would seem, can be adapted to almost any kind of heavy work. This suspended lift has been in use in each of its different forms for a year past in the new steel shop of the Nashua Iron and Steel Company at Nashua, N. H., and has uniformly given great comfort and satisfaction.

NOTE.—The above paper was in the hands of the secretary of the Institute for publication, and the proper papers were prepared to secure a patent, when the writer found that the hydraulic features of the above described "lift" seemed to be already covered by a previous patent for a "steam portable crane!" The lift was in use daily for nearly eighteen months, and was believed to be entirely novel. While the principles seem to be previously covered their application for the especial purpose described is believed to be novel.

IMPERFECTIONS IN SURVEYING INSTRUMENTS.

AN ENGLISH AND AN AMERICAN TRANSIT FITTED WITH THE IMPROVED TRIPOD HEAD, AND A MINER'S DIAL.

BY JOHN HENRY HARDEN, UNIVERSITY OF PENNSYLVANIA, PHILADELPHIA.

WITH imperfect instruments it is impossible to make accurate surveys; the results are inaccurate maps, with their attendant consequences. The design of the writer is to describe an improved form of tripod head, and certain defects in an English transit and a miners' dial, with the method taken to remedy the defects of the transit. When the present transit superseded the surveyor's compass, there was one special objection brought against it, and with some show of reason; namely, that the tripod of the compass with its ball and socket motion, allowed a quicker levelling-up (and, consequently, more work) than was possible with the transit. To this objection it was replied, that the ball and socket would not allow the instrument to be levelled with the same degree of precision that could be attained with the levelling screws, and that if time was sacrificed greater accuracy was attained. Nevertheless, it is a well-known fact that in this country, and especially in England, the compass or miners' dial holds its ground solely from its rapid adjustment.

Ever since the introduction of the transit, efforts have been made to give to this instrument the same facility of levelling possessed by the compass or dial, and the same means have been tried, *i. e.*, a ball and socket motion, since a modification of this in some form was the only method that would answer. The upper part of a transit is, however, much heavier than the corresponding parts of a compass, and, therefore, requires more binding power, which cannot be obtained with an ordinary ball and socket joint. It has been a study with instrument-makers to combine the facility of these two tripods —the quickness of the ball and socket with the steadiness and accuracy of the screws; but in all attempts in this direction the ball and socket of the compass has been literally followed, the entire stability of the instrument depending upon the friction of the ball in its socket.

There is great need for a tripod that can be more easily manipulated than the present form, for it rarely happens that an engineer has a level surface on which to set his instrument, and very often

the topography of the country, or the highly inclined strata of the mine, present places where it is often very difficult to set up an instrument. Much of the time in surveying is employed in this adjustment; and during this time the corps of assistants are generally kept waiting; it will be, therefore, readily understood that any change tending to give greater speed in the setting up and levelling operations is of no small importance.

The present form of tripod has many defects which have been borne with simply because no method has been devised to correct them. One of the disadvantages is, that it is almost impossible to level up a sensitive bubble, so that it will remain in the centre of its run long enough to take a satisfactory sight; that is, after setting the instrument level and sighting, on taking a second look at the bubble it is almost invariably found out of place. It is then necessary to level up again, and repeat this operation two or three times before one can feel satisfied that the observation is correct. These defects were thought to arise from the springing of the plates by forcing the levelling screws too tight; investigation, however, has shown that, in the majority of cases, there is one common cause, the springing of the screws themselves. These screws, when moved in or out to a considerable extent, do not stand perpendicularly to the plate on which they rest, but on an inclined plane, and on turning them, their points have a tendency to slide down this plane. (See Fig. XII, Plate VI, C. D.) In this position they spring, and turning them has a tendency to bind or bend them. If any two opposite levelling screws are, the one screwed and the other unscrewed (as in levelling up an instrument), the points of the two remaining screws will describe small arcs of circles instead of standing upon fixed points.

Another imperfection in some instruments is found in the parts forming separate planes, one through the axis of the instrument and the centre of the ball and socket, *M*, the other, through the points of the levelling screws, *H H H*, resting upon plate *I I*. This will be found illustrated in Fig. XVIII, Plate VI, in which the solid perpendicular lines, *H H H*, represent the screws resting upon plate *I I*; *A B* is the plane of the ball and socket *M*; the inclined dotted lines are positions of the tripod; the perpendicular dotted lines and the points *a a*, *b b*, *c c*, are the positions of the points of the screws, corresponding to the positions of the tripod. It will be observed that the levelling screws are not always equidistant from the axis of the instrument. In the diagram Fig. XIX, Plate VI, the above defect is in a great measure remedied by making the planes coincide.

There is yet another imperfection in the construction of some instruments; that is, the plummet is attached to some point on the axis above or below the centre of the ball and socket. In either case, the plummet, after being set over a station, will, during the operation of levelling up, travel away from the point. This will be found to operate in a degree proportionate to the distance of the attachment of the plummet from the centre of the ball, and the deviation of the axis from the perpendicular at the time the instrument is moved over the centre. This defect can be got over by levelling the instrument first, then moving it over the station. No such defects exist in the construction of the improved tripod to be described.

Although all the other parts of the instrument have been improved, from time to time, the tripod head, with one exception, remains the same as originally devised. The one change is the addition of the shifting-head, allowing the entire instrument, with its plummet, to be accurately placed over a fixed point, after the operation has been approximately performed by moving the legs. This improvement, in various shapes, common to all first-class instruments, is a valuable one, and is shown in Fig. I, Plate V. *I I* is the screw cap of the tripod; *J J*, the shifting plate; *H H*, the levelling screws. On unscrewing these, the plate *J J*, forming a part of the socket *N N*, of the small ball *O O* (the centre of which is the axis of the instrument, and the point from which the plummet is suspended) can be moved in any direction within the limits of the inside opening *L L*, of the screw cap *I I*, together with the plate *K K*, forming a rest for the levelling screws.

The advantages possessed by the new form of tripod head are as follows:

First. A saving of time, arising from an increased facility of setting-up and levelling, which can be done approximately at once without the use of the screws. Less than half a turn is then necessary to bring the instrument to a perfect level, the operation at the same time clamping the instrument. *Second.* The levelling screws, *H H*, are at all times perpendicular to the plate *G G*, to which they are attached, and the plate *K K* and screw cap *I I*, on which they rest. *Third.* The levelling screws are reduced in length, and their duty to a minimum, the instrument being no higher or heavier than before. The shifting head for plumbing over a fixed point is retained, no extra screws being required to clamp the instrument, this operation being performed at the time it is brought

over the point, and finally levelled by half a turn or less of the screws. When this is accomplished, the centre of the cross-wires of the telescope, the centre around which the instrument turns (being the centre of the two balls (*M*), the point from which the plummet is suspended), the point of the plummet, and the fixed point or station over which the instrument stands coincide with each other.

Fig. XI, Plate VI, is a cross-section of the improved tripod head, and Fig. XII is a section of the old form of tripod. In the former *I I* is the screw cap, *J J* the shifting plate, a part of the small socket *N N* of the ball *O O*, and *H H* are the levelling screws; these parts are common to all first class instruments. It will be observed, however, that besides the half ball and socket (*N N*, *O O*) above mentioned, there is an extra and larger ball and socket represented by the letters *P R*, *P R*, forming a part of plate *S S*, to which the instrument is fastened, and plate *G G*, to which the levelling screws are attached, the latter plate always remaining parallel to the screw cap of the tripod head, on which the points of those screws rest; so that whatever position the instrument may assume in relation to the tripod head the screws will always act directly perpendicular to both plates, *G G* and *I I*. The contrary is the case in the old form of tripod, as shown in the cross-section, Fig. XII, where the screws appear resting upon the inclined plane, *C D*. In Fig. XI the two half-balls, *N O*, *N O*, and *P R*, *P R*, are respectively $1\frac{1}{4}$ and $3\frac{1}{2}$ inches in diameter, and the instrument moves under all conditions upon a centre common to both balls, this being the point (*M*) to which the plummet is attached; it is therefore impossible for the plummet to be otherwise than perpendicular to the axis of the instrument.

It is scarcely necessary to give here the *modus operandi* of using the instrument fitted with this improvement, it will be readily understood from an examination of the drawings. The improved head saves from one-half to two-thirds of the time usually occupied with screwing and unscrewing as in the old plan.

Although mention of this improvement has been made only in connection with the transit, yet, it is equally adapted to theodolites, levels, and astronomical or other instruments of a like character. Figs. III, IV, V, VI, VII, and VIII, Plate V, are elevation, plan, and detail of a London transit owned by the writer, which, could it have been examined before purchase, would never have come to America. In two instances the fastenings, as illustrated by Fig. VII, failed to hold the instrument and tripod together while being carried

on the shoulder, and the instrument once fell to the ground. The device shown in Fig. VIII was designed to obviate this defect, but failed in its object, and it was then determined to have the instrument fitted with the improved tripod head. Fig. IX, represents the instrument after the alteration. If in the old form of the instrument it was required to be set up on uneven ground, as represented by Fig. III, the axis of the instrument could by no possibility be brought to coincide with the perpendicular of the plummet, in other words, the axis of the instrument could not be set perpendicular to a fixed point or station. A possible error from this source is illustrated by Fig. III; Fig. V, shows the rigid attachment hook from which the plummet is suspended, having no connection with the axis of the instrument. The dotted hook, Fig. III, is perpendicular to the axis of the instrument, that in solid line to the tripod and shifting bar *J. J.* Before the alteration, the instrument, complete on its legs, weighed 30¼ lbs., it now weighs 25½ lbs. Figs. XIII and XIV, illustrate the method of fastening the legs to the tripod.

An improved form of the tripod head designed by the writer is shown in Fig X. Its possible advantages over the original invention, are a greater sweep of the instrument (15° 30′) on each side of the perpendicular, a longer spindle or axis on which the instrument revolves, a larger diameter and in consequence, a larger bearing surface in the large ball. The levelling screws are covered from dust, and at the same time are no obstruction to the working of the instrument in any position it can be placed, and its greater width, equal to the distance from centre to centre of the levelling screws, gives it greater stability.

Fig. XVII, Plate VI, is a miners' dial imported by the writer. It illustrates very plainly a similar defect to that of the London transit, but in a more aggravated form. The drawing shows an error of more than three-fourths of an inch, from which there is no relief when setting-up on a sloping surface.

Figs. XV and XVI are elevation and plan, detail drawings of the large ball to transit Fig. X.

The drawings, one-half the natural size, were all made from actual measurements of the instruments. The letters and figures correspond with the same parts on all the drawings.

THE COAL AND IRON OF THE HOCKING VALLEY, OHIO.

BY T. STERRY HUNT, LL.D., F.R.S., MONTREAL, CANADA.

IT is now five years since I called the attention of the Institute to the industrial importance of the coal and the iron ores of the Hocking Valley in Southeastern Ohio, and in a pamphlet on the region published soon after, in 1874, endeavored to resume the principal facts then known respecting it, including the observations of the geological survey of Ohio, made and published up to the time by Professors Andrews and Wormley, together with an estimate of the probable future of the region, and its relations to the coal and iron markets of the country. Since that time the labors of the Geological Survey have included a further study of the coal by Prof. Edward Orton, the results of which will be awaited with much interest. I may say from my knowledge of them, that while confirming the general conclusions of his predecessors, he has been enabled to add much to our knowledge as regards the correlation of the various members in different parts of the coal-field, and to show more clearly the positions and the importance of the ore-beds in the Hocking Valley region. The only ore mined in that vicinity previous to 1874, was below what is called the Great Vein or No. VI coal, and to the west of what is known as the Hocking Valley coal-field, and although experiments in a few furnaces in the State had shown that this coal could be used with advantage in iron-smelting there was not a single blast furnace within the field. My observations at that time, however, impressed me with the abundance of iron ore, not only below but above this important coal seam, and I then ventured to express the opinion that this region, from its cheap and easily mined smelting-coal, its native ores and limestones, and its geographical position and relations to the great markets of the West, and to the supply of rich iron-ores from Lake Superior, was destined at no distant time to become a great metallurgical centre, where iron could be made more cheaply than at any other equally accessible point in the country.

The object of the present communication is to report progress, and to state how far my observations made in the region last month have justified the predictions of 1874. The five years which have elapsed have been, as all know, most unfavorable to the iron trade, and especially to new enterprises in untried regions, yet they have not passed without important industrial developments in the Hocking Valley, where not less than thirteen blast furnaces have been erected

within the past three years, the whole within a radius of about ten miles, and not far from Shawnee, Straitsville, and Bessemer. One of these is not yet completed, one being enlarged, and three, from financial difficulties, are inactive. The others are in successful operation, and it is probable that in a few months the whole number will be in blast. Of these furnaces the greater number use native ores alone, in some cases the Iron Point or Black Band, which after calcination, gives fifty per cent. of iron; while three are smelting, with admixtures of native ores, yielding from thirty to forty per cent. of iron, about one-fifth of Lake Superior ore. The success of those who have been able to carry on their smelting operations has been, for the most part, highly satisfactory, and the iron produced is of excellent quality, especially for foundry purposes, and finds a ready sale in the markets of Ohio and Michigan. The production, which amounted to about 24,000 tons in 1877, exceeded 70,000 tons in 1878, and the present improved tone of the iron-market will probably lead to a considerable increase in 1879. This record, it may be fairly claimed, shows a remarkable progress in a new region, effected during a period of unexampled depression, and is of good augury for the future.

I should violate confidences were I to give the figures privately communicated, or obtained from the books of the Akron and other furnaces, but it may be said that even at the lowest prices in 1878 the iron from these furnaces yielded large profits, and that the cost of production is very low. The net price paid for mining coal at the side of the furnaces is 43 cents per ton, and the royalty paid is 10 cents, and when the ores near by, yielding from 30 to 40 per cent. of iron, are mined by contract at from 60 to 70 cents, and at most $1 a ton, it is easy to see that the conditions are exceptionably favorable. The experience already gained, however, shows that here, as elsewhere, care must be exercised in the selection of ores, and also in the location of the furnaces with reference to a supply of water. The use in the furnaces of coke made from the coal seams just above the Great Vein will probably be found of considerable advantage when mixed with the raw coal from the latter for iron-smelting.

The exports of coal from the Great Vein in this region are considerable, and increasing in amount; a single railroad, the Columbus and Hocking Valley Railroad, in 1878, having carried of it 914,000 tons. Of this a large part finds its way to the lake ports, whence the ores of Lake Superior can be readily brought. These in the future, it may be expected, will not only be used in admixture

with the native ores, but, smelted by themselves with the cheap coal and coke of the region, will afford materials for the manufacture of Bessemer and open-hearth steels.

DISCUSSION.

MR. WILLIAM KENT (speaking of the coking properties of the coal) referred to an experiment made in the spring of 1877 at Shawnee, where a beehive coking oven had been erected, and an attempt made to coke a small lot of coal, which was said to have been brought from another portion of the district, ten or twelve miles distant. The reports of the result of this test were quite conflicting, but he had seen samples of the coke in Columbus, which appeared to be of excellent quality; and Mr. Fulton, of the Cambria Iron Works, had shown him a piece of the same, which he (Mr. Fulton) said was as fine a piece of coke as any in the country.

Thirteen blast furnaces have been built in the Hocking Valley within the past four years, and many hundreds of thousands of dollars have been spent upon them and in developing the ore and coal, yet not one thousand dollars has been spent to determine whether or not the coal could be coked—the most important problem before the district. The furnaces are all using soft coal, with the usual accompaniments of small product (12 to 25 tons per day), frequent scaffolds, and consequent high cost of labor per ton of iron. If the coal of the Bayley's Run seam can be coked, the furnaces may be built larger, and their capacity be increased to 100 tons per day. The cost of this coke should be but little more than that of Connellsville at the ovens where the latter is made, and as the coal is freer from ash and sulphur than Connellsville, its quality should be superior. With native ores and limestones immediately adjoining the coal, and with facilities for obtaining Lake Superior ores by competing lines of railroad as cheaply as they can be obtained at Pittsburgh, a hot-blast coke iron of any desirable quality might be made cheaper than at either Pittsburgh or Cleveland. All this, however, depends upon the practicability of making a good coke from this coal; and an experiment on a large scale to determine this question ought to be made preliminary to any further investment of capital in the district.

MR. ED. NICHOLS said that the Bailey's Run coal, which had just been spoken of as being suitable for coking, probably contained too much sulphur to be used for that purpose without washing, and that it was questionable whether sufficiently abundant supplies of water

for that purpose would be generally obtainable near the mines. In the matter of a constant water supply the Hocking Valley region was generally deficient, and a number of cases were mentioned where furnaces already erected had been forced to build dams across dry ravines to hold surface-water produced by rains, this being their only source of supply. The principal streams of the region, with the exception of the Hocking River, although furnishing considerable water during the spring and winter, dry up almost completely in the summer.

THE BRADFORD OIL DISTRICT OF PENNSYLVANIA.

BY CHAS. A. ASHBURNER, ASSISTANT, SECOND GEOLOGICAL SURVEY OF PENNSYLVANIA, PHILADELPHIA.

THE Bradford Oil District lies in the northern part of McKean County, Pa., and the southern part of Cattaraugus County, N. Y. Although petroleum was first found in the *producing sand* in 1871, it was not until the early part of the year 1875 that a productive horizon was acknowledged to exist, even by those most largely interested in the production of the crude oil.

It has been reported to me that the first well drilled in the valley of the Tuneangewant Creek (commonly called Tuna Creek) was drilled by F. E. Dean and brothers in 1865. This well was located on the Shepherd farm, near the present site of Custer City. One hundred and sixty feet of drive pipe was used, and the hole was drilled to the depth of 900 feet.

The *producing sand* at Custer City is found about 1130 feet below the level of the railroad track. The Shepherd farm well was therefore abandoned over 200 feet above the top of the oil sand.

The next well was drilled by the Dean brothers on the Clark farm at Tarport. Drilling was stopped at a depth of 605 feet, or over 400 feet above the top of the oil sand.

In the year 1862 the old Bradford well, since known as the Barnsdall well, was drilled to a depth of 200 feet with a spring pole, and then abandoned. In the spring of 1866 the citizens of the village of Bradford concluded to club together and sink the Barnsdall well deeper. It was drilled to a total depth of 875 feet, or to within 150 feet of the Bradford *producing sand*. All of these wells were drilled with the expectation of finding the Venango County oil, and at

about the same depth below water-level as at Oil City. They were all utter failures, and the old Bradford well, drilled to a depth of 200 feet in 1862, has as much claim to have been the first *oil* well in the Bradford District as any of those which were subsequently drilled by the Dean brothers.

The *first well* sunk to the Bradford sand was drilled by Mr. James E. Butts, Hon. C. H. Foster, and Mr. Job Moses, with a few others, under the name of the Foster Oil Company. This well was situated on the Gilbert farm, two miles northeast of Bradford. "Slush" oil was found at a depth of 751 feet, and the producing sand was struck at 1110 feet in the month of November, 1871. The daily production was 10 barrels. From the time when the sand was found in the Foster Oil Company's well to December, 1874, no wells were drilled that amounted to anything.

On December 6th, 1874, Messrs. Butts and Foster struck the oil sand in what is known as the Butts well, No. 1, on the Archy Buchanan farm, two and a half miles northeast of Bradford. This well started off with a daily production of 70 barrels, and was really the first well that attracted the attention of the oil men to the possibility of finding a profitable oil district in the county.

The unparalleled growth of the field is evidenced by the fact that in December, 1878, four years from the completion of the Butts well, the average daily production of crude oil was 23,700 barrels, or about $\frac{4}{7}$ths of the total daily production of the State of Pennsylvania.

Geological Position of the Bradford Producing Sand.—The early drillers in the territory regarded the oil as coming from the same geological horizon as that occupied by the "Third Sand" along Oil Creek, in Venango County. Inconsistent as this idea was with known facts in the geology of Northwestern Pennsylvania, the *producing sand* was named the "Third Sand," and the determination of a "First" and "Second" sand was left to the driller. No careful examination was made of the "sand pumpings," but from the way the drill pierced the strata two sand horizons were located. The upper sand, about 600 feet above the producing sand, was named the "First Sand;" the lower one, 300 feet above the same horizon, was named the "Second Sand."

The opinion which had been frequently expressed by expert geologists that there was little probability of finding the Oil Creek sands north of the Philadelphia and Erie Railroad was denied on the basis of what the driller regarded as overwhelming evidence. As far as nomenclature went the comparison between the Venango and McKean

County oil rocks seemed perfect. Along the Allegheny River, in the former county, the drill had proven the existence of three distinct sand horizons producing petroleum, which had long been known as the "First," "Second," and "Third" sands, the latter being the most productive.

One mile above Oil City, in Venango County, the top of the "Third Sand" is 528 feet above ocean level. At Bradford, which is 64 miles, north 55 degrees east, of Oil City, the top of the producing sand is only 414 feet above ocean level. If the producing sand in the two localities was the same, there would be a dip in the sand from Oil City to Bradford of (528–414) 114 feet. Between these two places the surface rocks have a persistent dip to the southwest, which averages about 14 feet per mile. This estimate is based on the identity of the Second Mountain Sand, the bottom of which north of Oil City is about 1290 feet above ocean level, with the Olean conglomerate, the bottom of which at Bradford is 2170 feet above the same datum.

The interval between the outcropping conglomerates and the geological horizon of the Oil Creek "Third Sand" probably varies but little between the two points, so that the stratum, whatever it be, which occupies the horizon of the "Third Sand" in McKean County should be found at Bradford at about water-level. But the top of the sand which produces the petroleum at Bradford is found 1030 feet below water-level.

In Venango County, according to Mr. Carll, Assistant in the oil regions, the average distance from the top of the "First Sand" to the bottom of the "Third Sand," is 315 feet. In McKean County, I find from a study of a number of well records, that the average distance from the top of the First Sand, so called, to the bottom of the so-called "Third," or producing sand, is 660 feet. Here then are two facts which cannot be denied, if the Oil Creek "Third Sand" is geologically the same as the Bradford sand: First, the Bradford sand is over a *thousand feet* lower than facts would lead us to suppose, and, second, the groups of oil rocks in McKean County are over *three hundred feet* too thick. Again, if the Venango and McKean oil sands were the same, of course the whole rock series would have to thicken very much to the northeast.

The accompanying chart of columnar sections (see Plate VII) shows that the formations between the Olean Conglomerate and Bradford Oil Sand, thicken very materially from Bradford south to Ridgway. If the strata from the Second Mountain Sand to the

Oil Creek "Third Sand" did not remain approximately constant from Oil City to Bradford, as we have supposed, there would be many more reasons to assign a thickening to the southwest rather than to the northeast.

Mr. Carll, in the early part of 1876, published the fact that the Bradford *producing sand* was probably 1000 feet below the Oil Creek "Third Sand." Facts since obtained show this to have been a close estimate.

To make a comparison of the rocks passed through in the two districts, it was necessary to have complete and authenticated records. No accurate register of the rocks has ever been kept by any of the producers in the Bradford district. This fact can readily be accounted for when it is remembered, that with the exception of the wells at State Line and Limestone, N. Y., the bulk of the production comes from one horizon. The difference in the strata is so slight, that except by a close examination of the sand pumpings, it is impossible to distinguish any change in the succession of the sedimentary deposits.

In December, 1877, Professor J. P. Lesley appointed Mr. Arthur Hale to the special work of obtaining a correct record of the Dennis & Co.'s well, No. I, which was about to be drilled on the high summit about three-quarters of a mile southwest of Bradford. This well was completed in the early part of 1878. The measurements were made with great care, and wherever the rock was found to change a specimen or specimens were secured for future study. The depth of the well is 1719 feet. The elevation of the floor of the derrick above ocean is 2055 feet, Bradford station on the Erie Railway (now N. Y., L. E. and W. Railway) being 1444 feet. This record is without doubt the *longest detailed and accurately measured* record of any oil well in the United States. Deeper wells have been drilled, but no record has ever been kept so accurately as this one to such a depth. A complete description of the record is contained in a paper which I read before the American Philosophical Society, September, 1878.

The top of the well is stratigraphically 115 feet below the bottom of the Olean Conglomerate, which is the lowest member of the Coal Measure conglomerate, No. XII, and which caps the highest summits in the vicinity of Bradford.

The Mauch Chunk shales, No. XI, if present in this part of the county, are represented by the shales immediately underlying the Olean Conglomerate. They cannot be more than 5 or 10 feet thick.

The strata pierced in this well may be grouped as follows:*

Pocono (Vespertine), No. X, . . .	132 feet.
Catskill (Ponent), No. IX, . . .	250 "
Chemung (Vergent), No. VIII, . .	1337 "
Total, ...	1719 feet.

The total thickness of No. X is about 230 feet, allowing 7
for the shales of No. XI, immediately underneath the Olean.

The following is a condensed description of the Dennis reco

Pocono Formation, No. X.

	Thickness.	Depth.
Surface clay, ...	4 feet to	4 feet.
Olive gray shale, ...	11 "	15 "
Gray sandstone, ...	33 "	48 "
Gray shale, ...	19 "	67 "
Blue and gray sandstone, shale and slate, .	49 "	116 "
Gray sandstone, ...	16 "	132 "

Catskill Formation, No. IX.

	Thickness.	Depth.
Red shale.	6 feet to	138 feet.
White and gray sandstone, ...	59 "	197 "
Red shale "Paint Rock,"	18 "	215 "
Gray sandstone containing a few pebbles, .	23 "	238 "
Bluish slate, ...	77 "	315 "
Gray sandstone, ...	5 "	320 "
Red slate, ...	8 "	328 "
Gray sandstone and slate, ...	39 "	367 "
Red shale, mottled, ...	15 "	382 "

Chemung Formation, No. VIII.

	Thickness.	Depth.
Gray slate, ...	8 feet to	390 feet.
Dark and gray sandstone, ...	45 "	435 "
Fine sandstone and slate, ...	216 "	651 "
Gray sandstone and slate, ...	61 "	712 "
Red sandstone, ...	10 "	722 "
Dark slate, ...	20 "	742 "
Sandstone and chocolate shale, ...	63 "	805 "
Gray slate and sandstone, ...	201 "	1006 "
Red slate and shale, ...	14 "	1020 "
Gray sandstone and slate, ...	36 "	1056 "
Gray and yellow sandstone, "First Sand," so called, ...	25 "	1081 "
Gray sandstone and slate (oil show in lower part), ...	44 "	1125 "
Gray slate, ...	175 "	1300 "

* The classification of the rock series here indicated is provisional. Fo
final arrangement of the strata the reader is referred to my forthcoming r
on McKean and Elk counties, Second Geological Survey of Pennsylvania.

	Thickness.	Depth.
Brown sandstone, ...	17 feet to	1317 feet.
Slate, ...	28 "	1345 "
Brown and gray sandstone, "Second Sand," so called, ...	36 "	1381 "
Gray slate, with occasional sand beds, . .	283 "	1664 "
Brown sand, "Third Sand," so called, or Bradford *producing sand*, ...	54 "	1718 "
Slate and sandstone, ...	1 "	1719 "

Three hundred and twelve specimens were obtained of the strata encountered in the Dennis well.

For convenience of study I have placed a portion of each specimen in a homœopathic vial, half-inch diameter. These vials are placed on their sides, and piled one upon another, and placed in three walnut cabinets, each cabinet containing 104 bottles. Each bottle is separated by a small strip of tin from the adjoining bottle, so that any one may be removed for examination without disturbing the others. The space in each cabinet which contains the samples is 5¾ feet long and 2½ inches wide.

To the right of the rock column I have placed a columnar section drawn to scale, so that the specimens may be referred directly to their vertical position in the well record. It will be noticed that the bottles themselves are not placed according to scale, but are piled directly one upon another, irrespective of the interval which separates them in the record. If the specimens had been placed to scale the total length of the section would have been 45 feet instead of 17¼ feet as at present. The size of the cabinets would have been awkward, and no material advantage would have been gained. I have given the above description, from the fact that I believe it to be the best method for the study of well records in connection with specimens of the borings.

The *producing sand* in the Dennis well is 54 feet thick. The sand is of about the same degree of coarseness as the ordinary beach sand along the Jersey coast, and contains comparatively little cementing material. The sand is finer and closer in texture than that of any other oil-producing territory in Pennsylvania. It is more homogeneous in section, and has a more constant character over a wider area than any other oil-sand.

These facts have much to do with the small percentage of risk which the driller experiences of obtaining "dry holes." The Bradford is the surest and safest district in which to operate.

In January last there were 112 wells completed in the territory,

of which 8 or $7\frac{1}{10}$th per cent. were dry holes. The percentage of dry holes in the other districts of the State was $29\frac{1}{10}$th, or 7 wells out of 24 completed.

In the Bradford district, from December 1st, 1877, to December 1st, 1878, there were 2018 wells completed, of which 87 were dry holes, or $4\frac{3}{10}$ths per cent. The percentage of dry holes in Pennsylvania, exclusive of the Bradford district, during the same year was $24\frac{7}{10}$ths.

In the Haskill well at Smethport, fifteen miles southeast of Bradford, a sand was struck at 1345 feet, and was reported to be 12 feet thick. The sand which at present is producing about two barrels of oil a day in the Haskill well was found at a depth of 1718 feet, and is 18 feet thick. Drilling was continued to 124 feet below the lower or producing sand. The Smethport producing sand, by most of the producers, is considered to be the same as the Bradford sand. From a careful study which I have made of a number of surface sections and well records in northern McKean County, I have come to the conclusion that the upper sand in the Haskill well is the representative of the Bradford sand, and that the lower or producing sand in this well lies 360 feet geologically lower than the great productive horizon of the Bradford district.

The Smethport oil horizon is interesting, from the fact that it is the *lowest* geological horizon at which petroleum has been found in Pennsylvania. I first announced the discovery of this fact in a paper, which I read before the Engineers' Club of Philadelphia, February 16th, 1878, on the "Oil Sands of Pennsylvania." At that time, the sand had not been found at Smethport, but had been pierced by the drill in a well at Sartwell in an adjoining township.

The grouping of the rocks of McKean and Elk counties, under the head of our well-known Palæozoic formations, can only be done, at present, provisionally. The surface of the territory is so universally covered with drift, and the few rock exposures which are found are so scattered that the classifications of the strata have been as various as the reports which have been published.

It may be interesting to state, in this connection, that there are as many as five distant and independent sandstones in McKean and Elk counties, which by different authorities have been considered to be the representative of the Pottsville, or Coal Measure Conglomerate, No. XII. The practical result has been that in McKean County alone not one-half of the lands which have been negotiated as un-

derlaid by valuable coal beds, will ever produce one ton of *commercial coal* at such a price as to render the development profitable.

The confusion which has resulted from attempted classifications of the sub-conglomerate rocks has been no less than that which has come from an erroneous identification of the conglomerate itself. My examinations in the northern part of the State have so far been confined to a district in which no one of our Palæozoic formations has been absolutely determined. The grouping which has been suggested in this paper seems to be the most natural one, in view of the resemblance of the section to those in distant, but carefully studied portions of the State. It was approved and adopted by Professor Lesley, after comparing my sections with the conclusions reached by Mr. Andrew Sherwood from his studies in Potter and Tioga counties, which lie directly east of McKean County. Whether our future examinations will verify this provisional classification or not, it is of no importance as bearing on the practical conclusions of this paper. The upper red bands, shown in the section at Bradford, at Smethport, at the Wilcox wells, at the Bear and Silver Creek wells, and at Ridgway, are unquestionably the same. Whatever formation they may prove to be a part of at Ridgway, they will be a part of the same formation at Bradford. A careful examination of the fossil forms in this district from the Chemung up into the lower productive Coal Measures has been made, but the means which the fossils afford for stratigraphical determinations are extremely limited.

Palæontologically considered, the rocks from the base of the Olean Conglomerate, No. XII, to the bottoms of the deepest valley, some 800 feet in all, are essentially one group incapable of subdivision. They contain in all eighteen Waverly species, seven Chemung species, and one of Carboniferous type. The Waverly species do not seem to predominate above, nor the Chemung species below any fixed horizon. I have grouped the rock series mainly from lithological determinations, which undoubtedly lead to the most reliable and accurate conclusions, when sufficient sections can be had for comparison. Having determined the relation of the producing sand at Bradford to the overlying strata, I next sought for some constant horizon which should afford the best means of comparing distant sections. This was of importance in order to ascertain the approximate depth below the surface of the Bradford sand in any territory adjacent to the developed district, in which petroleum should be drilled for. The rock most constant in its gen-

eral character in McKean and Elk counties seems to be the Olean Conglomerate.

Between this conglomerate and the oil-bearing sand there is no stratum or series of strata, with possibly the exception of the red shale bands of the Catskill, No. IX, and Chemung, No. VIII, which can furnish a reliable guide to the oil prospector.

The sandy measures in the 1000 or 1500 feet immediately overlying the oil-sand at Bradford, are poor guides in looking for the oil-sand in new or wild-cat territory. They lead to confusion, error, and disappointment. There is no guide which the driller considers more infallible than the so-called "First" and "Second" sands. It is true that in a limited territory, there are distinct sand strata 300 and 600 feet respectively above the Bradford sand, but I believe it impossible to determine the position of the oil-sand by an arbitrary location of these upper sands.

One important fact, which is too often overlooked, is that the rocks may thicken or thin between two constant horizons in comparatively short distances, so that allowances must be made, either plus or minus, in estimating the proper depth to drill in order to strike the producing sand.

The chart of columnar (Plate VII) sections shows the position of the Bradford producing sand below the Olean Conglomerate at Bradford, in the Haskill well at Smethport, in the Wilcox wells, in the Bear Creek well, in the Silver Creek well, and in the old Dickinson or Ridgway well. The horizon of the sand in the three last wells is probably about the same depth below the bottom of the Red Catskill as it is in the Wilcox wells. Although the bottom of the Olean Conglomerate in the vicinity of these different wells is found at varying elevations, yet I have placed it in the drawing on the same horizontal line, for convenience of comparing the underlying strata.

It will be noticed that the Pocono formation, No. X, and Red Catskill, No. IX, thicken very much south from Bradford. As a consequence the Bradford oil-sand horizon at Ridgway would be found nearly 600 feet further below the Olean rock than it is at Bradford. This fact has a very important practical bearing. The Bear Creek well, which was drilled to a depth of 1998 feet, and the Silver Creek well, which is 1760 feet deep, have both been abandoned long before the Bradford sand could possibly be reached. I do not mean to say that if these wells were drilled to the proper depth, they would prove productive. Not at all, but I do assert

	Distance in miles from Dennis well.	Direction from Dennis well.	THICKNESS OF FORMATIONS, IN FEET. No. XI.	No. X.	No. IX.	No. VIII, to top of Bradford oil-sand	Elevation of top of well.	Depth to top of Bradford oil-sand.	Elevation of top of Bradford oil-sand.	Total depth of well.
McKEAN COUNTY.										
Dennis well, Bradford	....	....	No. XI, in McKean Co., has not been absolutely recognized, but is probably represented by 5 to 10 feet of shales under the Olean Conglomerate.	250	247	1282	2055	1664	391	1719
Hulings well, Kinzua Creek	13.39	S. 8° 30′ E.		....	....	....	1625	1545	80	1650 (?)
Smethport well	13.77	S. 49° 00′ E.		260	....	....	1590	....	....	2001
Haskill well, Smethport	14.92	S. 44° 30′ E.		260	250	1305	1552	1345	207	1861
Hukill well, N.E. of Wetmore	18.72	S. 44° 30′ W.		343	260	1299	1846	1950 (?)	104	2011
Wilcox well, No. 3	21.25	S. 4° 00′ W.		330	263	1290	1666	1685	19	1850
Coburn well	21.65	S. 10° 45′ W.		325	260 ±	1281 ±	1900	1944	44	2263
Ernhout and Taylor, No. 2	22.80	S. 11° 00′ W.		340	....	....	1730	1880	150	2000
ELK COUNTY.						No. VIII, to bottom of well.				
Bear Creek well	29.37	S. 4° 30′ W.	45	520	813	1055	1595	Not reached.	....	1998
Silver Creek well	29.92	S. 6° 00′ W.	45 ±	....	....	797	1615	"	....	1760
Ridgway well	36.8	S. 6° 40′ W.	45 ±	675	....	70	1383	"	....	772

The distance drilled below the top of the Bradford sand may be found by subtracting the depth to the top of the sand from the total depth of the well.

that they should be much deeper to strike the Bradford sand if it underlies this portion of Elk County.

These two wells form examples of many that I could cite where the money spent in drilling has been more than thrown away. They prove nothing and only tend to condemn the territory as "dry" without any facts to support such a conclusion. Can it be denied that geological work is of practical use to the oil prospector?

The table on the preceding page gives the thickness of the several formations shown in the chart (Plate VII), together with the tidal elevation of the Bradford sand. The well records have been continued up to the bottom of the Olean Conglomerate, where the drilling was commenced below that horizon, by surface observations.

The geological position of the top of each well in McKean County, in the table, may be found by subtracting the distance to the top of the Bradford sand from the sum of the overlying formations. In making the same estimate of the wells in Elk County, the total depth of the well must be taken instead of the distance to the top of the Bradford sand, since this horizon was not reached by the drill.

A section is not given on the chart of the Hulings, Hukill, Coburn, or Ernhout & Taylor (No. 2) wells.

It will be noticed that between Bradford and Smethport the strata from the Olean rock to the Bradford sand maintain almost a constant thickness. From Bradford southwest to the Hukill well, which is one and a half miles northeast of Ludlow station, the Pocono, No. X, thickens about 100 feet, while the other formations remain about the same as in the Dennis well. In the Wilcox wells, No. X is about the same thickness as in the Hukill well. The greatest amount of change in the two counties takes place between the Wilcox wells and the Bear Creek well. In this distance of $8\frac{1}{4}$ miles the interval from the bottom of the Olean to the top of the Catskill thickens to the south at the rate of $28\frac{1}{2}$ feet per mile. The same rocks between the Bear Creek and Ridgway well thicken to the south at the rate of $26\frac{1}{2}$ feet per mile. From the Wilcox wells to the Ridgway well the Red Catskill, No. IX, thickens at the average rate of $4\frac{1}{2}$ feet per mile.

If it was not for the geological fact that the formations thicken in Elk County rapidly to the south, the Bear Creek well would be deep enough to have encountered the horizon of the Bradford sand.

The elevation of the bottom of the Olean Conglomerate in the vicinity of the Wilcox wells is 1879 feet; at Ridgway the same geological horizon is 1746 feet above tide, so that the *average dip* of the bottom of No. XII between the two places is about $8\frac{1}{2}$ feet per mile.

The southern dip of all the rocks below the Olean would be greater than $8\frac{1}{2}$ feet per mile on account of their thickening to the south.

Dip of the Bradford Sand.—The producing sand from the Four Mile district, which is in Cattaraugus County, N. Y., about seven miles northeast of Bradford, toward Bradford averages 12 feet per mile, south 50° west. From Bradford toward Shepherd Run the average dip per mile is 11 feet, south 25° west. From Tarport north to State Line, to Limestone, the dip varies from 15 to 19 feet per mile to the north. This change of dip to the north near the State Line has doubtless influenced the shape of the valley eroded by the Allegheny River.

The development which has been made at State Line and at Limestone is outside of the line which I have drawn on the map as the approximate boundary of the Bradford district, in October, 1878. The wells at these two places have been excluded because their characteristics are quite different, and they seem to form an outlying patch of the main district. The total length of the Bradford territory proper, northeast and southwest, is over 17 miles, while its maximum breadth northwest and southeast is about 8 miles.

Production of the Bradford District.—The growth of this oil-field has been so rapid and so different from the other districts in Pennsylvania that I have thought it might be of interest to the Institute to make some general statements as to its production.

DAILY PRODUCTION OF BRADFORD DISTRICT COMPARED WITH THE TOTAL DAILY PRODUCTION OF THE STATE.

		State.	Bradford.	Proportion.
1874,	December,	27,682 bbls.	75± bbls.	
1875,	June,	23,207 "	125± "	
	December,	23,254 "	149 "	
1876,	June,	24,120 "	800 "	$\frac{3}{100}$
	December,	25,390 "	1,800 "	$\frac{7}{100}$
1877,	June,	37,693 "	3,449 "	$\frac{9}{100}$
	December,	40,518 "	8,000 "	$\frac{19}{100}$
1878,	June,	40,575 "	16,000 "	$\frac{39}{100}$
	December,	42,538 "	23,700 "	$\frac{56}{100}$

Three years ago from last December Bradford produced less than one-hundredth of the total production of Pennsylvania. In December, 1878, Bradford produced over one-half of the entire production of the State.

The following table gives the total production in the State and the Bradford district for 1876, 1877, and 1879:

	Bradford.	State.
1876, ...	382,768 bbls.	8,968,906 bbls.
1877, ...	1,465,481 "	13,135,671 "
1878, ...	6,208,746 "	15,165,462 "
Total for three years,	8,056,995 bbls.	37,270,039 bbls.

The total production of McKean and Cattaraugus counties from the discovery of petroleum in 1871 up to the first of the present year, would amount in the aggregate to about eight millions one hundred thousand (8,100,000) barrels of oil.

The average daily production of the 3000 wells in the Bradford district during last month (January) was 25,000 barrels or 8⅓ barrels per well. The average daily production per well of those which were completed during the month was 17½ barrels.*

A METHOD OF ROLLING STEEL OR IRON EYE-BARS.

BY CHARLES MACDONALD, C.E., NEW YORK CITY.

WROUGHT-IRON eye-bars for bridges and roofs, designed upon what is known as the pin connection system, have been successfully manufactured in this country for some years. The most approved methods employed have been the hydraulic upsetting process, and by die-forging under a hammer.

In upset eyes the fibre of the iron is necessarily much distorted; and in bars of considerable size, this disturbance seriously impairs the strength of the connection.

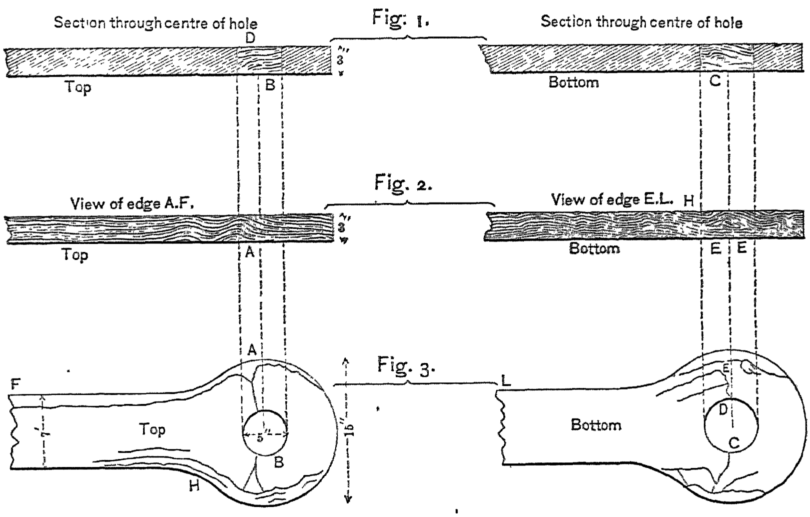
Mr. Collingwood, in discussing my paper on "Proportions of the Heads of Eye-bars," read before the American Society of Civil Engineers, Jan. 21st, 1874, submitted sketches of certain bars intended for the anchorage of the East River bridge, which had been exposed to the action of sulphuric acid a sufficient length of time to thoroughly cleânse the surfaces, and expose the condition of the fibre. These sketches are here reproduced for the purpose of illustrating the effect of this method of manufacture. In Figs. 1, 2, and 3, it will be observed that the fibres at A, and E, have been folded back upon themselves and otherwise twisted and disturbed, in a manner not calculated to inspire confidence in the integrity of the connection. To compensate for this defect, a large excess of metal is thrown into the eye; but a method of mânufacture which involves such a necessity, even although it be the one giving the best results thus far, all things being considered, is far from bèing perfect.

* My conclusions are based on statistics taken from Stowell's Petroleum Reporter of Pittsburgh.

Die-forged bars, when *well* made, are more uniform in the disposition of material about the pin-hole; but the extra expense involved in the manufacture, practically rules them out of the market. There are die-forged eye-bars, so called, which are made by welding an extra piece or pieces on the ends of bars, and hammering into the *shape* of an eye; but it is needless to say, that these cannot be included in the category of *well* made eyes.

By neither of these processes, however, has it been found possible to make uniformly, good *steel* eye-bars; and it has been due to this fact, perhaps more than any other, that the value of this material, manifestly so much better suited for structural purposes than iron, has not yet been fully recognized in bridge construction.

The want, above referred to, would seem to have been met by an invention recently perfected by Mr. Andrew Kloman, of Pittsburgh, by which he is enabled to roll either steel or iron eye-bars direct from a billet, without upsetting or in any way disturbing the continuity of the material.



The machine by which this result is accomplished is, to speak in general terms, a universal roll train, so arranged that the lower horizontal roll can be raised or lowered by hydraulic power applied to toggle-joint connections on the under side of the bearings. The upper roll is moved by screws in the usual way, and by a convenient disposition of the pitch any required vertical movement can be secured by revolving the gearing a given number of teeth.

In operating the machine, a billet of the required weight to insure proper length of finished bar, is first rolled into the form indicated in Fig. 4, a squeeze being given at each reversal of the rolls by first dropping the lower roll, then screwing down the upper one a given amount, and then forcing the lower roll back into its normal position in which the arms of the toggle-joints are vertical. When the required dimensions of the body of the bar have been thus obtained, the unfinished ends are reheated and rolled into the flat blanks represented on the left extremity of Fig. 5, by passing them under the rolls at right angles to the direction in which the bar was finished: this is provided for in the arrangement of the housings opposite the power end of the rolls. The finished eye, as indicated at the right end of Fig. 5, is finally sheared out in a hydraulic press. As might have been supposed, the operation of reheating, and cross rolling at the ends, renders it necessary to anneal the bar after the eyes have been formed; this however is accomplished at very slight expense,

Fig. 4.

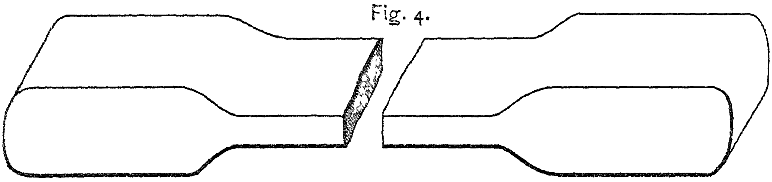
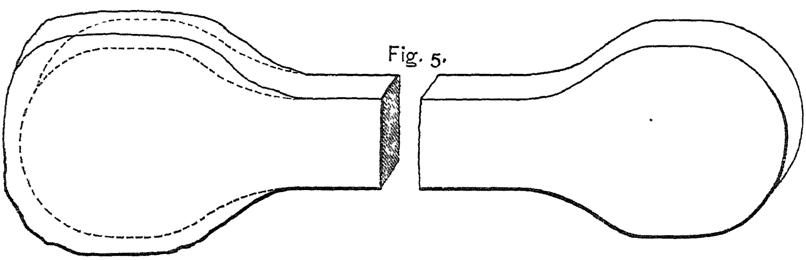


Fig. 5.



and the general results, so far as may be known at present, are highly satisfactory.

All the eye-bars for the Glasgow Bridge, now being constructed over the Missouri River, at Glasgow, Mo., were rolled in steel by this process to the satisfaction of Gen. W. Sooy Smith, Chief Engineer. As the entire work is not yet complete Gen. Smith's report upon the behavior of these bars under strain may be delayed somewhat, but that the chief impediment in the way of introducing steel as tension

members in bridge construction, has been removed by Mr. Kloman's very ingenious contrivance for rolling eye-bars, can no longer be seriously doubted; and it does not require a prophetic inspiration to predict that within the next few years a new impetus may be given to the production of steel by reason of an increasing demand for structural purposes.

52 WALL ST., NEW YORK, Feb. 18th, 1879.

THE LAKE SUPERIOR COPPER ROCKS IN PENNSYLVANIA.

BY J. F. BLANDY, PHILADELPHIA.

IN October last, I was call upon to examine a copper deposit in the South Mountain, near the Pennsylvania and Maryland boundary. The specimens shown me contained oxides and carbonates with native copper, and were similar to some that had been described to me some years ago as existing at the Luray Gap in Virginia.

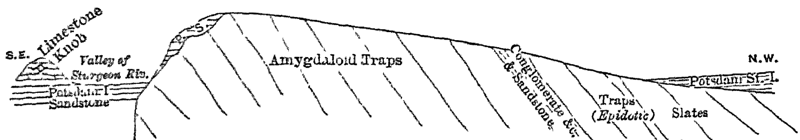
An examination of *Rogers's Report on the Geology of Pennsylvania*, led me to expect to see his so-called "Primal Formation," such as exists throughout the southeastern district of the State. I was therefore much surprised on arriving at the summit of the Western Maryland Railroad to find myself on familiar ground. Instead of the "Primal" as I had known it in Southeastern Pennsylvania, I soon decided that the rocks were identical with the "Lake Superior Copper Formation." My visit was unfortunately a short one,—part of two days, and in that time I had to pass over a good many miles,—but I saw sufficient to convince me that instead of the Lake Superior Copper Formation being a local geological feature, it would have to be considered as a widely extended series of rocks. I was unable to even approximately estimate the thickness either of the trap zone or other members, schists and slates, of the series.

In an article on the Copper Range of Michigan, prepared by C. P. Williams and myself in 1862, and published in vol. xxxiv of the *American Journal of Arts and Sciences*, we endeavored to divide that range into zones, according to the character of the traps. The limited nature of the explorations at that time made this work very imperfect. Subsequent explorations have confirmed my notes then

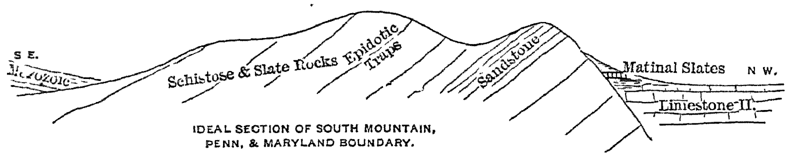
made, but at the same time have added so much information, that a much better subdivision could be made now, and other zones added to the list then given. It was there stated, that the epidotic traps (amygdaloids) lay nearest to the base of the range. Since then a highly epidotic zone has been found nearly overlying the great central conglomerate and sandstone, and near what was then thought to be the top of the series.

I refer to these points from the fact that I found much of the traps of the South Mountain to be very epidotic in character, and being succeeded by schistose and slate rocks, I judge the series there exposed to belong to the upper part of the formation.

I feel that it would be absurd to attempt to established any parallelism in order of deposition between two localities so far separated, and only give these points for what they may prove to be worth. Still such parallelism may be possible. Sections across the two ranges in conjunction with other formations bear a much better comparison, and below I give them in explanation.



IDEAL SECTION OF TRAP RANGE OF LAKE SUPERIOR, HOUGHTON COUNTY.



IDEAL SECTION OF SOUTH MOUNTAIN, PENN. & MARYLAND BOUNDARY.

At Lake Superior we have the belts of traps and overlying slates dipping to the N. W., with the Potsdam Sandstone lying unconformably on the upturned edges of the trap to the southeast. In the valley of the Sturgeon River are two knobs, remnants of the Limestone (II of the Pennsylvania Series). At South Mountain we have the traps with the overlying schists and slates dipping to the S. E., with the Matinal slate (III) lying unconformably on the upturned edges of the series to the N. W. The lowest member of the series in South Mountain that I noticed was a heavy-bedded sandstone. This may be the equivalent of the great central conglomerate and sandstone of the Lake Superior range. The upper members of the series overlying the central conglomerate, are but little known in the Lake district, as they are to such a large extent

either covered by the waters of the Lake or by a deep deposit of drift material. Much that has been written of them is conjecture, and it cannot yet be proven that they lessen in dip towards the N. W., *until they become horizontal and conformable with the overlying Potsdam Sandstone.*

This upper part of the series is that which is well exposed in the South Mountain; the lower part, that which is most conspicuous at the Lake, is here hidden by the overlying limestone, but may show itself better farther southward. How far to the north and south these rocks may be traced becomes an important question. They undoubtedly extend as far north as Carlisle, and probably southward to North Carolina, in a continuous belt. The separating of this series from the "Primal" of Rogers may enable us to solve many of the difficulties which have puzzled geologists in Southeastern Pennsylvania.

In this connection I would further call attention to a statement set forth in a paper on "Topography" by me and published in Vol. I of this society's *Transactions*, in which I showed that the S. E. side of the Lake Superior copper range, had been elevated since the Drift period some 300 to 400 feet, causing without doubt a fracture at the base of the ridge. Can there be any coincidence in date between this fracture and those immense ones known to exist along the line of the valley of Pennsylvania and Virginia? The present Geological Survey of Pennsylvania will in time give us some light on this subject.

I will not at present make any reference to the copper deposits of South Mountain, hoping to do so at another time, with more extended information.

DISCUSSION.

Dr. T. Sterry Hunt objected to Mr. Blandy's view which made the cupriferous rocks of the South Mountain, south of the Susquehanna, equivalent to the Keweenian series, which yields the native copper of Lake Superior. He had, in 1875, spent some days in an examination of this part of the South Mountain in company with Professor Persifor Frazer, Jr., and had briefly described these rocks in 1876. Mr. Blandy's reasoning was, as the speaker understood, based chiefly on the mineralogical resemblances between certain greenstones and epidotic rocks there associated with copper and copper ores, and the rocks accompanying the native copper of Lake Superior. From his familiarity with

the rocks of both regions, however, the speaker expressed the opinion that these resemblances were but superficial, and that the differences, even from a lithological point of view, were very great. These Pennsylvanian rocks were by the speaker referred to the Huronian series, and had been by him described as a great development of petrosilex or orthophyre, often porphyritic, "distinctly bedded, presenting different varieties, and alternating with dioritic or diabasic, epidotic and chloritic rocks (greenstones), and with argillites, in which are sometimes included thin beds of the petrosilex; the strata generally dipping at high angles to the southeast." (Proc. Amer. Assoc. Adv. Sci., 1876, p. 211.)

The resemblances between these indigenous Huronian greenstones, and the so-called greenstones and melaphyres of the Keweenian series of Lake Superior led Logan in 1848, and Rivot in 1856, to confound the copper-bearing series of Lake Superior with the Huronian rocks of that region, but the distinctness of the two was pointed out by Whitney in 1857, and confirmed by Kimball in 1865. Whitney, however, failed to distinguish between the Keweenian and the overlying Paleozoic sandstones, called Potsdam, and united them in one group, which he declared to be stratigraphically distinct from the underlying crystalline (Huronian) rocks. Later, in 1873, however, Brooks and Pumpelly looked upon the Keweenaw rocks as conformable with the Huronian, and probably more closely related to these than to the Potsdam. The speaker, after some time spent in the study of these various series of rocks on Lake Superior, in 1872, had found clear evidences of a stratigraphical break between the Huronian and the Keweenaw series, and in a communication in 1873, to the Institute of Mining Engineers, maintained that the source of the copper in the latter was to be found in its solution from the copper-pyrites, which abounds in the Huronian, and its precipitation among the materials of the newer series, for which he then claimed the rank of a distinct formation, independent alike of the Huronian and the Potsdam, and proposed for it the name of the Keweenaw group. (Trans. Amer. Inst. M. E., vol. i, pp. 339, 341.) In 1875, Major Brooks, having, apparently independently, reached the same conclusion, announced the existence of a stratigraphical break between the Huronian and the Keweenaw series (which he had shown to be distinct from the Potsdam), and proposed for it the name of Keweenawian, for which the speaker, in 1876, suggested the more euphonious adjective, Keweenian. The later researches of the Geological Survey of Wisconsin, by Irving, fully confirm these conclu-

sions, and show that the Lower Cambrian (Potsdam) Sandstone, with its characteristic fauna, rests horizontally upon the upturned edges of the Keweenian. The conglomerates of the latter, in various localities both on the north and south shores of Lake Superior, abound in the ruins of the Huronian greenstones, quartzites, and chloritic schists, as well as of various Laurentian and Montalban rocks, as elsewhere described by the speaker. Boulders of the characteristic petrosilex rocks already noticed, also abound, and, as at the Calumet and Hecla, and the Boston and Albany mines, make up the great mass of the cupriferous conglomerate of the Keweenian. The copper-bearing rocks of the South Mountain have the characters not of the Keweenian, but of the Huronian series.

The Keweenian occupies, in the opinion of the speaker, the same geological interval as the Taconian (Lower Taconic, Emmons), represented by the Primal and Auroral strata of the Great Valley in Pennsylvania, with which, however, it has no lithological similarity, and cannot, in the present state of our knowledge, be correlated. The copper which, in the form of sulphurets and silicates, is occasionally found in the Taconian, as for example, at the Cornwall and Jones mines in Pennsylvania (Trans. Amer. Inst. M. E., iv, 326), was perhaps derived from the decay of the pre-existing Huronian strata, and a similar source is probably to be assigned to the copper so generally disseminated through the Mesozoic strata of Eastern North America, in the form of native copper or of rich sulphurets, as copper-glance. This is the result of a chemical concentration from the cupriferous pyrites of the older rocks, from which, by oxidation, the copper is removed in a soluble form, leaving the iron behind as an insoluble oxide.

The Pre-Cambrian rocks in Wales, to which Hicks has given the name of Pebidian, have all the characters of the Huronian. The petrosilexes which in North America the speaker had included in the base of the Huronian, are absent in several localities where this rests on the Laurentian, leading to the suspicion of a want of conformity between them and the great mass of the Huronian. The similar petrosilexes in Wales, were by Hicks at first united to the underlying Dimetian (Laurentian?) gneisses, but from late evidence obtained in South Wales, Hicks was led, in 1878, to separate them from the rocks above and below, and to make of them an intermediate group, stratigraphically distinct alike from the underlying gneisses, and the overlying Huronian (Pebidian) greenstones and schists. To this new series, consisting chiefly of petrosilex rocks, he has given the name

of Arvonian. The speaker, who had lately examined with Hicks, the principal localities of Pre-Cambrian in Pembrokeshire, Caernarvonshire and Anglesea, regarded the Arvonian as the equivalent of the petrosilex rocks of the South Mountain in Pennsylvania, of Eastern Massachusetts and New Brunswick, of Lake Superior, Wisconsin, and Missouri. The Pebidian of Hicks is the Huronian of the Atlantic belt, of Lake Superior, and of the Sierra Nevada. The Montalban series, represented by the gneisses and mica-schists of Philadelphia and New York, is not known in Wales, but is well developed on the opposite coast of the Irish Channel, where it forms the Dublin and Wicklow hills, along the southeast coast of Ireland, and appears to be directly followed by the Lower Cambrian sandstones and slates.

For a detailed account of the chief points involved in the history of the Keweenian and Huronian rocks the speaker referred to his Historical Introduction to the Study of American Pre-Silurian Rocks, published in 1878, by the Second Geological Survey of Pennsylvania; Report E, part i, pp. 70–80, 189–194, 219–241.

Mr. Blandy said he did not base his argument upon the presence of epidote in the traps (only referring to that and other points of comparison for what they might prove to be worth), but upon the whole section of the formation, and the large development of the trap rocks. It was true that trap occurred in the Huronian, but almost always in the form of dikes, and not in extensive beds as at the South Mountain. It was not necessary that all the rocks that were found in the South Mountain series should have their equivalent at Lake Superior. The two points were so far apart that we might well expect to find in the one varieties of rock which would not be found in the other.

Professor P. Frazer, Jr. (At a subsequent session).—In the numerous sections which accompany Report C C, 1875 (Second Geological Survey of Pennsylvania), it is everywhere clearly shown both in the text and in the graphic illustrations, that the Potsdam or Primal Formation of Rogers is wanting over all that country, with the exception, perhaps, of detached patches on the N. W. flank of the South Mountain chain.

The following few extracts may serve to show this (C C, p. 278): "Description of a route over the South Mountain from Petersburg to Boiling Springs." The rocks here encountered were "floating boulders of sandy schist;" "jaspery quartzite, or orthofelsite;" "compact schists;" "a sandy greenish laminated rock intersected by quartzite, the layers ¼ inch thick;" "crystalline schist, etc." "This would

seem to show not merely a non-conformability between the older (Huronian?) orthofelsites and schists and the more recent (Cambrian?) sandstone, but it would seem additionally to imply that the alignment of one system was the result of causes entirely different from, and anterior to those that formed the other."

The text of section No. 8, after including a list of the rocks and dips, among which the Primal is nowhere mentioned, continues (p. 284): "This section presents a few noticeable features which serve to corroborate the structure as interpreted from other sections" (*i.e.*, that the N. W. underlying half of the chain consists of quartz conglomerate schist, and the overlying eastern half of orthofelsite schists, etc.)

On p. 285 occurs the following language (see also the section):

"Hence it is apparent that the great South Mountain chain is composed essentially of two groups of rocks, the lower (and along this line the northwestern) consisting of various modifications of the quartz conglomerate above spoken of, in which quartzite occurs in various forms. The upper and southeasterly group is felsitic in character, but contains also large beds of hydromica and chlorite schists," etc., etc., etc.

So in Section X, near Caledonia Furnace, p. 292: "The character of the rock is either conglomerate or quartzite for nearly five miles. At this point it becomes blue in color, and is replaced by orthofelsite porphyry," etc.

So much for the published reports of the survey which have already, in the most emphatic manner, separated the South Mountain series from the Primal of Rogers. In Nos. 16 and 17, vol. iii of the *Polytechnic Review* (April 21st and 28th, 1877), is an article by me on the "Copper Ores of Pennsylvania," which is largely concerned with the description of the very locality referred to by Mr. Blandy. On p. 159, No. 16, it is stated: "But a far more interesting series of occurrences of copper ore is to be found inside the eastern bounding chain of the South Mountain, and extending from Millerstown down to and beyond the Maryland line. The ore-belt lies in orthofelsite, which forms this portion of the chain."

Page 170, No. 17, sums up as follows, after condensing the more important conclusions from Owen's, Whittlesey's, and Shumard's reports in the survey of Wisconsin, Iowa, and Minnesota: "This description" (of the Lake Superior Copper Region) "is not quite applicable to the rocks of that portion of the South Mountain which we have been considering, and yet there are some resemblances which must be more than accidental. Throughout the South Mountain

range (viewed as a whole), from the Chambersburg turnpike to the Maryland line, we have an area of trapezoidal shape, of which the larger side rests on the above turnpike, and the shorter on the State boundary. The rocks of this region may be divided into two great series: a western (*underlying*), of which the characteristic strata are composed of quartzite and of arenaceous schist containing quartz pebbles ('Mountain Creek Rock'); and the eastern (*overlying*) of hydromica and chlorite schists, and orthofelsite, both porphyritic and unporphyritic. Both these series show indications of having been penetrated by dikes of plutonic character within this area. The porphyry, which carries the copper in this region, shows no character of igneous action, but occurs in coarse and thin beds, more or less disintegrated, and in certain localities reduced almost to the state of kaolin. Nothing which might correspond to the term sandstone was observed, though all the above sediments are full of grit and sandy particles. . . . . It seems fair to conclude that the region of the copper-bearing rocks belongs to the Huronian cycle, as do the similar porphyries in Missouri, and on Lakes Superior and Huron," etc.

I have quoted thus much to show that if Mr. Blandy is wrong in supposing the copper regions of the South Mountain and of Lake Superior to be of the same age, he is not the first to have fallen into that error. But I based my hypothesis on Dana's statement (*Manual of Geology*, 1875, p. 159), as I have never been in that region.

In the section representing the structure of the South Mountain, and appended to Mr. Blandy's paper, there are several points which my observation has failed to establish:

1st. The indefinite but evidently wide extension given to *beds* of "epidotic traps."

2. The occurrence of a large deposit of underlying *sandstone*.

3. The unconformable (or other) contact of Matinal slates to the N. W. and above the limestone.

4. No epidotic or trap-rocks have been noticed by me near the valley where the Matinal slates are said to lie unconformably. This portion of the chain, where not covered by a decayed sandstone (as in Col. Weistling's sand quarry, near Mont Alto), consists of a conglomerate of translucent or amethystine quartz in a matrix of bedded crystalline schist.

Mr. Blandy says, "They (the upper part of the series) undoubtedly extend as far north as Carlisle, and probably southward to North Carolina."

The whole wide series of orthofelsites, in which the copper belt lies in Southern Pennsylvania, seems to disappear before reaching the Potomac. A section along that river, from three miles west of Harper's Ferry to the Point of Rocks, made with Mr. Hall three years ago, failed to reveal this formation. The rocks which made Harper's Ferry are of the "Mountain Creek" type, the chloritic schists and hydromicas are the only representatives of what is, above the Maryland line, the eastern half of the South Mountain chain. It is true that these rocks may be cupriferous, but the writer does not know this fact.

DR. T. STERRY HUNT expressed his concurrence with the views of Prof. Frazer, with whom he had an opportunity of examining the rocks of the region in question, and referred for a further exposition of his opinions regarding them to the remarks which he made in the previous session, directly after the reading of Mr. Blandy's paper.

INDICATOR CARDS FROM A WATER-PRESSURE BLOWING ENGINE, WITH A NOTE ON A PROPOSED IMPROVEMENT IN SUCH ENGINES.

BY FRANK FIRMSTONE, GLENDON IRON WORKS, EASTON, PA.

THE indicator cards shown herewith were taken by the writer in June, 1877, from the water-pressure blowing engine of the Longdale Iron Co., at Lucy Selina Furnace, Longdale, Virginia.

A description of this engine with figures, and some indicator cards, was published by Washington Jones, Esq., Superintendent of the I. P. Morris Co.,* in the Journal of the Franklin Institute for May, 1877, to which those desiring a full account are referred, but for convenience the principal dimensions, etc., are given below.

The machine consists of a pair of horizontal direct-acting blowing engines, connected at right angles to a common fly-wheel shaft. The blowing cylinders are 4 ft. diameter by 5 ft. stroke; the water cylinders were 18 in. diameter by 5 ft. stroke as built. Both water and blowing cylinders are double acting. The water valves are balanced piston valves, 18 in. diameter, moved directly by eccentrics on the fly-wheel shaft. Great care was taken to have the areas of the ports and passages at least equal to the area of the working piston. The engine is set about 14 ft. above the tail-race, and the

* The engine was built by the I. P. Morris Co., from drawings made by Messrs. Taws and Hartman.

BACK END, WATER CYLINDER, DOWN-STREAM ENGINE

Revolutions per minute, 10¼.
Blast pressure 3 to 3¼ lbs. by gauge.
Mean head on piston, 77.05 feet.
Scale, 31.67 lbs.=1 in.

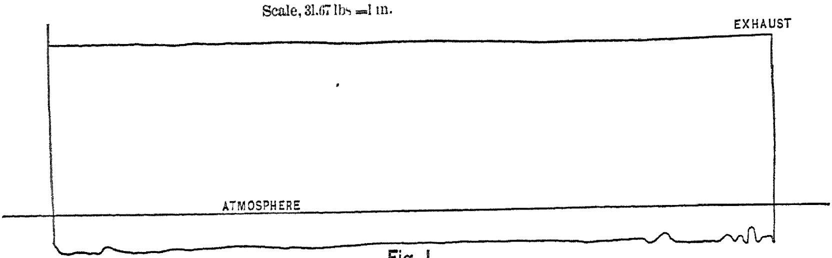


Fig. 1.

CRANK END, WATER CYLINDER, DOWN-STREAM ENGINE

Revolutions, per minute, 10¼
Blast pressure 3 to 3¼ lbs. by gauge.
Mean head on piston, 77.26 feet.
Scale, 31.67 lbs.=1 in.

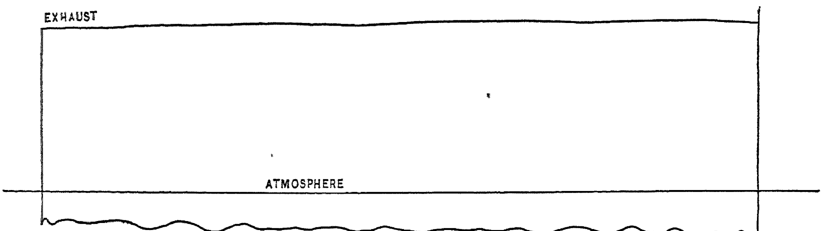


Fig. 2.

BACK END, WATER CYLINDER, UP-STREAM ENGINE

Revolutions per minute, 12.
Blast pressure 2¾ to 3 lbs. by gauge.
Mean head on piston, 75.88 feet.
Scale, 31.67 lbs.=1 in.

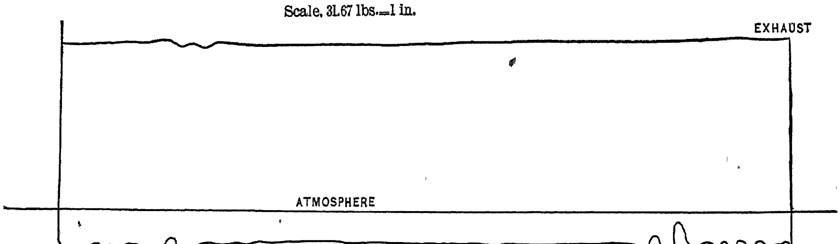


Fig. 3.

CRANK END, WATER CYLINDER, UP-STREAM ENGINE

Revolutions per minute, 12½.
Blast pressure by gauge, 2¾ to 3 lbs.
Mean head on piston, 72.38 feet.
Scale, 31.67 lbs.=1 in.

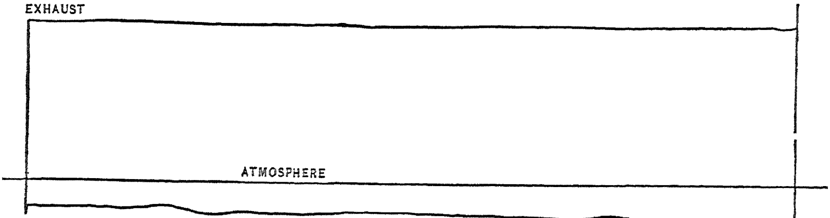


Fig. 4.

BACK END, BLOWING CYLINDER, UP-STREAM ENGINE

Revolutions per minute, 12.
Pressure of blast by gauge, 2¾ to 3 lbs.
Mean pressure on piston, 2.77 lbs per sq in.
" head " " 6.37 feet of water.
Scale, 5.44 lbs.=1 in.

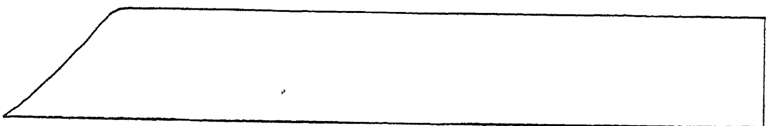


Fig. 5.

CRANK END, BLOWING CYLINDER, DOWN-STREAM ENGINE

Revolutions per minute, 16.
Pressure of blast by gauge, 3 to 3¼ lbs.
Mean pressure on piston, 2.93 lbs.
" head " " 6.74 feet.
Scale, 5.44 lbs.=1 in.

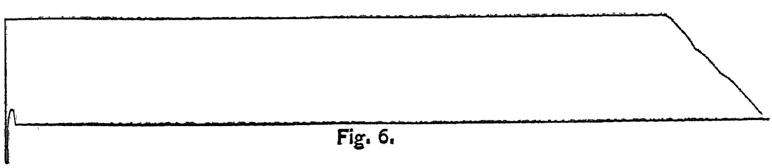


Fig. 6.

exhaust pipes are made to dip into the water in it, whereby the whole fall is utilized; the water in the exhaust pipes acting by suction, as is shown by the exhaust line in the cards falling below the atmospheric line.*

Soon after the engine had been set to work, it was found that the useful effect was higher than that assumed in calculating the diameter of the water cylinders, and that the desired pressure of blast (three pounds per square inch) could be obtained with smaller cylinders, and thus the volume of air delivered when the water in the stream was low, be correspondingly increased.

The diagrams were taken after the water cylinders had been reduced to $15\frac{3}{4}$ in. diameter by bushing. In all of them, the throttle valve on the pressure-pipe was wide open. Several cards were taken from both ends of all the cylinders, but as they show no important variation, only one from both ends of each water cylinder, and one from each blowing cylinder is shown here.

The indicator springs were tested against a column of water, and the scale thus obtained directly in *feet of water*. The corresponding pressures have been calculated by taking the weight of a cubic foot of water as 62.5 pounds.

The total head available has been assumed in all the diagrams as 77 ft., that being the difference of level between the water in the tail-race, and the waste trough on the supply-tank, but as the engine ran rather irregularly, because of the furnace being slightly scaffolded, the water sometimes rose three or four inches over this trough and sometimes fell as much below it. 77 ft. is therefore probably a little too high in the cards taken at 12 revolutions and somewhat too low in those taken at 10. It should also be noted that an error of about $\frac{1}{100}$ of an inch in measuring the mean height of a card from the water cylinders, would result in an error of 9 in. in the corresponding effective head on the piston.

The water diagrams show that the action of the valves is nearly perfect.

Dividing the mean effective head on the piston, as measured on the diagrams, by the actual difference of level from the supply-tank to the tail-race, we get the efficiency of the water cylinder, assuming that there is no leakage, and subject to the errors of measurement

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* This arrangement was supposed to be novel when the engine was designed, it was, however, patented in England in 1799. See Ewbank's Hydraulics, page 358. Ed. 1842.

above noted. This varies, from 100 to 93.5 per cent. in the different cards, or in other words there is little loss except from leakage.

Unfortunately I had no time to gauge the water, and thus determine the leakage. I would suggest to any one who may put up such a machine, that a permanent weir be placed in the head or tail-race. By keeping a record of the height of water on this weir, any unusual leakage of pistons or valves could at once be detected, and this in the long run, would probably pay for the head lost by the weir. This was not thought of when the supply-tank for the Longdale engine was arranged, and numerous springs in the tail-race prevented any satisfactory gauging in it.

The blowing cylinder cards show that there is ample inlet area and no appreciable loss from clearance and waste room

By dividing the power consumed in the blowing cylinder, by the power developed in the water cylinder, we get the efficiency of the machine, the loss being the friction.

Strictly speaking the two cards compared should be taken simultaneously. Having only one indicator with me, I could not do this, but have selected diagrams in which the speed, and the blast-pressure, as shown by the blast-gauge, were as nearly as possible the same.

In this way the efficiency of the *machine* is found to vary from 77 to 84 per cent, or a mean of about 80, and the useful effect of the water, neglecting leakage, from 100 x 80 to 93.7 x 80, or from 75 to 80 per cent. About 20 per cent. of the whole power therefore is lost in friction, which seems excessive.

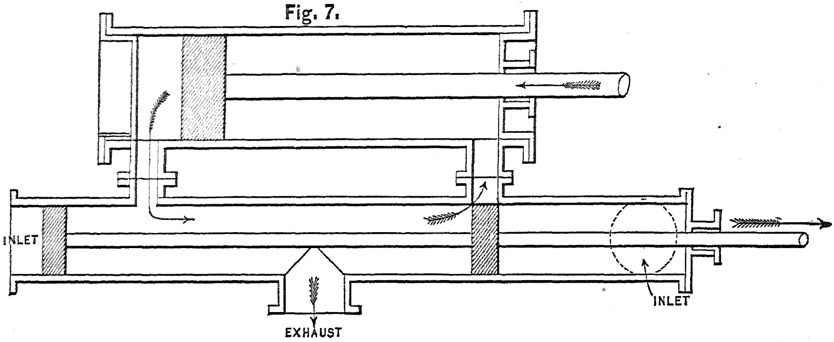
The above figures, although only approximate, show, as was to be expected, that the water-pressure engine is a very advantageous apparatus when high falls have to be utilized for blowing machinery. When arranged as the Longdale engine is, they have, however, one serious drawback, viz.: they are not adapted to give a varying blast-pressure economically. By bushing the Longdale cylinders, we made it impossible to blow more than about three pounds pressure of blast, no matter how desirable this might be by reason of any derangement of the furnace, even when there was plenty of water in the stream to do it, without diminishing the number of strokes of the engine per minute below the minimum due to the least supply of water in the stream, and the 15¾ inch cylinders.

One remedy for this inconvenience was proposed by Robert Briggs, C. E., in the *Journal of the Franklin Institute* for July, 1877, consisting in abandoning the direct-acting engine for a beam engine, whereby the stroke of the water cylinder could be made variable,

and thus the water consumed per stroke, and the pressure of blast be correspondingly altered, without altering the stroke of the blowing-piston. It seems possible, however, at least within moderate limits, to retain the direct-acting engine, and accomplish the same result, by the use of a properly designed variable cut-off.

If the water be cut off before the end of the stroke, and at the

Fig. 7.



same instant the exhaust be opened, the engine will complete its stroke (if the fly wheel be heavy enough) without further expenditure of water—that displaced in front of the piston flowing into the other end of the cylinder to fill the void which the piston would

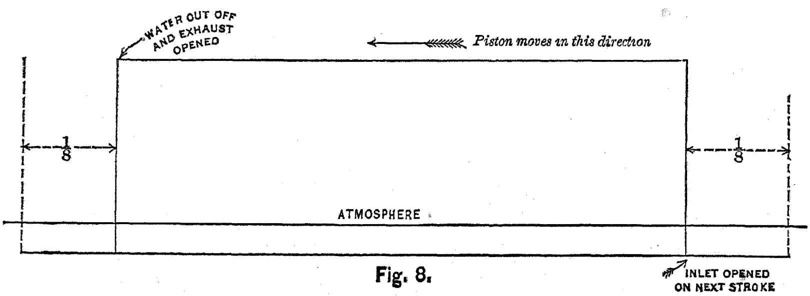


Fig. 8.

otherwise leave behind it (Fig. 7). Mr. John Maddock, Superintendent of Machinery at the Glendon Iron Works, suggested to the writer, that this could easily be accomplished with piston valves, by making the pistons movable on the valve stem, by means of right and left hand screws or other similar contrivances. The theoretical diagram for the Longdale engine, arranged to work by that means to three-fonrths of the full cylinder capacity (which would have produced the same results, if applied to the 18 in. cylinders, as was accomplished by $15\frac{3}{4}$ in. bushings) is shown in Fig. 8.

In fact, by moving the valve pistons apart, we give a certain amount of lap to the valves, and at the same time an equal amount of inside clearance. As the eccentrics are set without angular advance, if the water be cut off at $\frac{7}{8}$ of the stroke, the inlet will not be opened again until $\frac{1}{8}$ of the next stroke is completed, thus reducing the length of the cylinder to be filled with water by $\frac{1}{4}$. (Figs. 7 and 8.) Unfortunately, if we preserve full port openings when cutting off, this plan requires a very great travel for the valve—about 27 in. for ports $4\frac{1}{2}$ in. wide, cutting off at $\frac{7}{8}$ stroke. The use of excessively large eccentrics could be avoided in double engines connected at right angles, by moving the valves of each engine off the cross-head of the other, but double beat valves, moved by cams, would probably prove better than piston valves in any event, for although the water-passages cannot be made as direct as with piston, or other forms of slide valves, this would probably entail no serious loss, if care be taken to have them of sufficient area, and with a cam motion there is, theoretically at least, no difficulty in cutting off at any point in the stroke. There would be no danger of leakage from unequal expansion with such valves working in water, and I feel certain that they would be much tighter than piston valves giving equal inlet area.

Discussion.

In reply to a question of Mr. Holley about the weight of the fly wheel required if the cut-off was used, Mr. Firmstone replied that he had made no calculations, but should judge that four or five tons would be heavy enough for the Longdale engine, cutting off at seven-eighths stroke. Mr. Holley mentioned that in a steel rail mill with engine 46 in. × 4 ft., and 60-ton fly-wheel, it had been noted that it took 117 horse power to run the engine with train uncoupled. This, however, was abnormally high, due perhaps to the shaft being slightly out of line.

In reply to a question as to valves, Mr. Firmstone replied that there had been no difficulty with sticking, the water keeping them at a uniform temperature.

Mr. E. D. Leavitt, Jr., said: At the Calumet and Hecla mine we reduced the power required for the air compressors when their full capacity was not required by means of a set of valves on the air cylinders, which were operated by cams in such a manner that compression did not commence until a part of the stroke had been

performed. A similar device would answer for the water-pressure blowing engine.

In regard to the power required for fly wheels, we have at the Hecla mine a line of heavy shafting, driven by a large compound engine, on which there is one 20-ton, one 15-ton, and one 11-ton fly-wheel, a friction-wheel weighing 7 tons, four friction-pinions of 2 tons each, and an 8-feet pulley of 4 tons. Some 60 feet of the shafting is 12 inches, and 40 feet, 9 inches diameter. The indicator cards from the engine, when running at full speed with all the shafting on, averaged 48 horse power—actually $42\frac{2}{3}$ horse power at 54 revolutions per minute as the average of cards taken October 26th, 1878.

NOTE ON THE DETERMINATION OF SILICON IN PIG IRON AND STEEL.

BY DR. THOMAS M. DROWN, LAFAYETTE COLLEGE, EASTON, PA.

IN experimenting in connection with Mr. P. W. Shimer (now chemist of the Thomas Iron Company, Hokendauqua, Pa.) on methods for the determination of silicon in pig iron, in order to find one which should be accurate and yet give results in a few hours, I have adopted the following procedure, which, as far as my experience goes, leaves nothing to be desired.

About one gram of pig iron or steel is treated in a platinum or porcelain dish with 25 cubic centimeters of nitric acid (sp. gr. 1, 2). When action has ceased, 25 to 30 cubic centimeters of dilute sulphuric acid (one of acid and three of water) are added, and heat applied until the nitric acid is nearly or quite driven off. The heat of a water-bath is sufficient, though the process may be hastened by heating higher on a sand-bath. Water is then cautiously added (as soon as the free sulphuric acid is sufficiently cool) and the contents of the dish heated until the crystals of ferric sulphate are completely dissolved. The solution is then filtered *as hot* as possible, the residue washed first with hot water, then with 25 to 30 cubic centimeters of hydrochloric acid (sp. gr. 1, 12), and finally with hot water. After drying and igniting, the silica will be found to be snow-white and granular.

The following are some results obtained by this method compared

(in some instances) with the older method of treatment with nitric acid, evaporation to dryness, heating to 150° C. for several hours, dissolving out the iron in hydrochloric acid, and filtering off from the insoluble residue, which is dried and ignited, and the resulting impure silica fused with alkaline carbonates.

The letters denote different samples of pig iron.

	Per Cent. of Silicon.							
	A	B	C	D	E	F	G	Bessemer steel.
Old method	2.64	2 46	....	1.45	1 65	....	....	.672
New method, 1	2.70	2.47	1.13	1.63	1.53†	1 66	2.50	.676
" " 2	2 68	2.47	1.18	1.62	1.51†	1.68	2 50	.672
" " 3	2 81*	2 47	....	1.65	1 51†	1.72	2.50	.672
" " 4	....	....	....	....	1.63‡	1.70	2.47	....
" " 5	....	....	....	....	1.65‡	....	2.46	....
" " 6	....	....	....	....	1.65‡	....	....	....

* Not washed with hydrochloric acid.

† In these analyses hydrochloric acid was used after the addition of nitric acid, and was not completely driven off.

‡ Hydrochloric acid was used for solution instead of nitric acid.

Some incidental results obtained in developing this process have enough interest to be worthy of record. Treatment of pig iron with concentrated sulphuric acid, heating till fumes arise, diluting with water, and filtering after all action has ceased, gives a silica which is seldom pure, and yet the results are considerably too low.

Treatment with dilute sulphuric acid and evaporation till the acid fumes in the air, then filtering after dilution, gives occasionally results which are accurate; but this method is uncertain, depending on the fineness of the borings and character and composition of the pig iron. The silica obtained is seldom white. The following are some results obtained in this way:

	Per Cent. of Silicon.			
	A	B	C	D
Old method	1.02	1.64	2.64	3.85
New method, 1	1.05	1.73	3.00	3.88
" " 2	1.05	1.69	2.98	3.91
" " 3	1.05	1.70	2.97	....
" " 4	....	....	3.01	....

Treatment in platinum dishes gave very slightly lower results than porcelain dishes.

If after treatment with dilute sulphuric acid the solution is filtered off from the residue without concentration of the acid, it is found that about one-half of the silicon is in the solution and the other half in the residue; when nitric acid is used and the solution filtered off as soon as all action has ceased, it is found that about two-thirds of the silicon is in the solution and one-third in the residue; and with hydrochloric acid, about one-third goes into solution and two-thirds remains in the residue. It is not probable that there is any precise ratio existing between the amount of silicon dissolved and the amount in the residue in the case of any one of the acids, the ratio being doubtless variable and depending on the concentration of the acid, the time of action, and the temperature; yet the marked difference in the action of the three acids in this respect is interesting.

The washing with hydrochloric acid of the residue obtained by the action of nitric and sulphuric acids on pig iron is in most cases necessary. Thus there was obtained from a pig iron when water only was used for washing, 2.67 per cent. of silicon against 2.52 when washed with hydrochloric acid; and in another sample, 2.10 per cent. against 1.70 per cent.

Although the results obtained with hydrochloric acid for the original solution of the iron show, as far as the experiments go, as good results as those obtained with nitric acid, yet I prefer the nitric acid treatment on account of the silica obtained being compact and granular, while the use of hydrochloric acid, and also of sulphuric acid alone, yields a silica which is light and flaky.

EASTON, PA., February, 1879.

SKETCHES OF THE NEW MINING DISTRICT AT SULLIVAN, MAINE.

BY C. W. KEMPTON, MINING ENGINEER, WEST SULLIVAN, MAINE.

IF New England were located in some distant and almost inaccessible region, there is no doubt that its mineral resources would have been ere this well developed and generally acknowledged, but laboring under the disadvantage of nearness, it has been neglected. Its surface, moreover, is not cut up by mountains and cañons, and the remnants of many of the old ridges are extensively covered by glacial drift.

Having for the past ten months been engaged in the development of a mineral district in Southeastern Maine, I would take this opportunity to lay before the Institute sketches of results, and try to give some idea of the formations as found.

The Sullivan Mining District extends from the town of Franklin, through Hancock, Sullivan, and Gouldsborough, from northwest to southeast, about sixteen miles. A general idea of this locality may be gathered from the map accompanying. (Fig. 1).

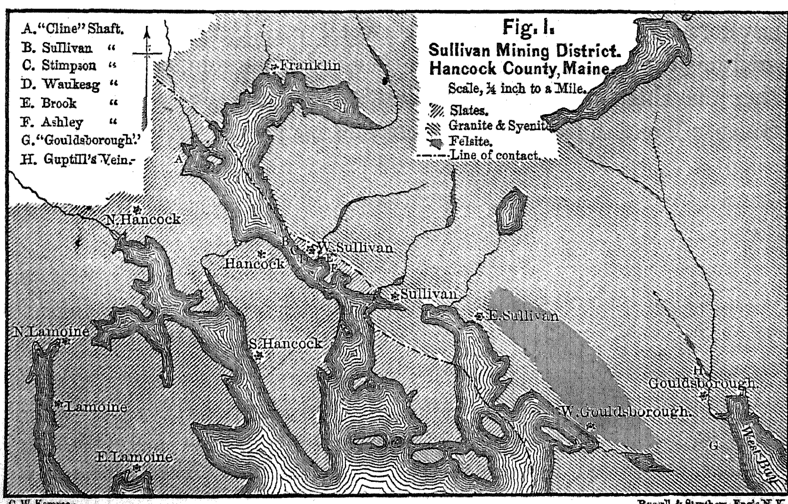


Fig. I. Sullivan Mining District. Hancock County, Maine. Scale, ¼ inch to a Mile.

Sullivan is situated at the head of Frenchman's Bay, ten miles north from Bar Harbor, Mount Desert, twelve miles from Ellsworth,

and thirty-seven miles from Bangor. It may be reached in summer by steamer, via Rockland and Bar Harbor, and has good hotel accommodations. A daily mail stage connects with Ellsworth and Bangor at all times of the year.

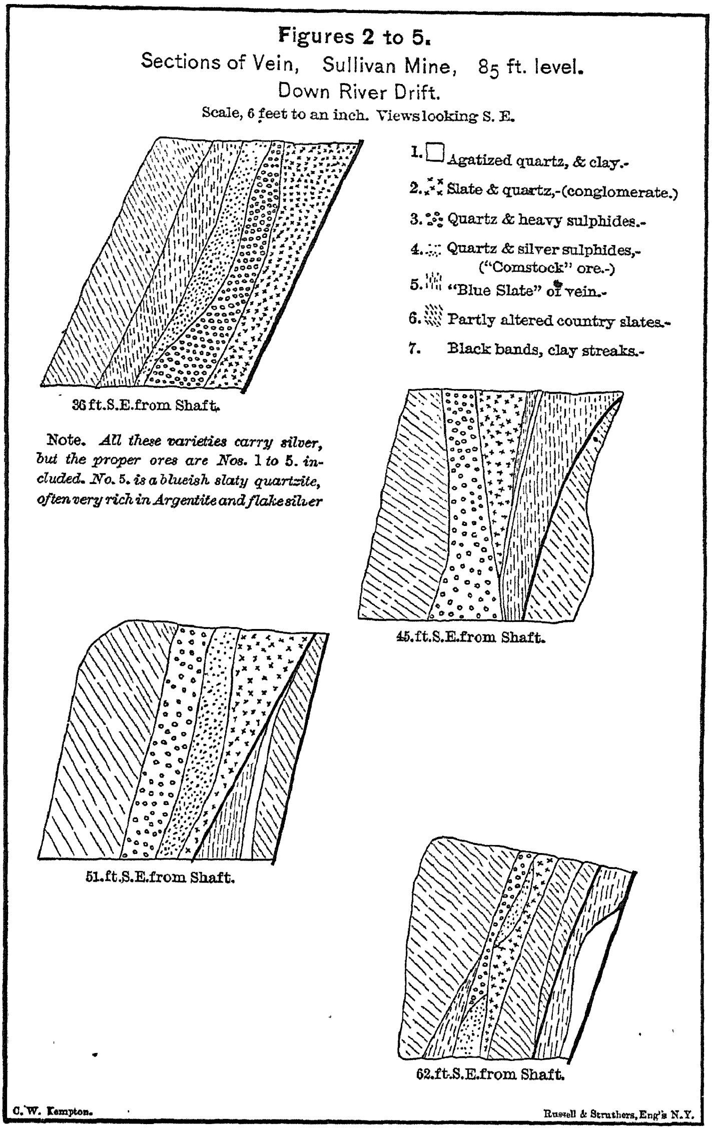
The staple production of Sullivan is granite, of which unlimited quantities of very fine quality may easily be obtained. At first impression a stranger would say that no other rock existed there, but further examination discloses the fact that the region has more slate than granite, and that along or near the line of their contact are most of the mineral veins of the district. The lithological characteristics of the surface are shown on the map. The slates of the region are quartzitic, of unknown age, undoubtedly very ancient, and referred to the later Laurentian—the limbo to which most of the doubtful New England rocks are consigned. Through these have been thrust the immense mass of granite and its allied rocks, from which action has proceeded the formation of the principal mineral deposits.

The first discovery of silver ores here was made by Mr. A. A. Messer, in May, 1877. They were discovered below high-water mark, on the shore of the bay, at the site now occupied by the shaft of the "Sullivan" mine. The vein, when found, showed about ten inches of quartz, carrying pyrites, galena, and traces of brittle silver (stephanite).

A coffer dam was built and shaft commenced. The first native silver threads were found about ten feet down. The vein was uncovered in several other places, also below high-water mark, proving that the showing at the shaft location was the poorest yet found. Seventy-five feet from the shaft, southeasterly, the quartz is four feet wide at three feet below the beach, and the lowest assay I have known from this is over $200 per ton in silver. The ore is principally stephanite.

Proceeding with the shaft, at about thirty feet depth, the vein, composed of quartz, with more or less slate highly impregnated with sulphides, was found to be four feet wide. At this time the shaft is about 100 feet deep. Drifts have been run 62 feet southeast from the 75 feet level, and 30 feet northwest from the 85 feet level. Sections showing the formation of the vein, and to some extent the nature of the ore, are given in Figures 2 to 9 inclusive. About 600 tons of ore are on the surface ready for concentration. The ore is essentially silver, sulphides and native, in quartz and slaty gangues, with slight amounts of iron, zinc, etc., as sulphides, and also galena.

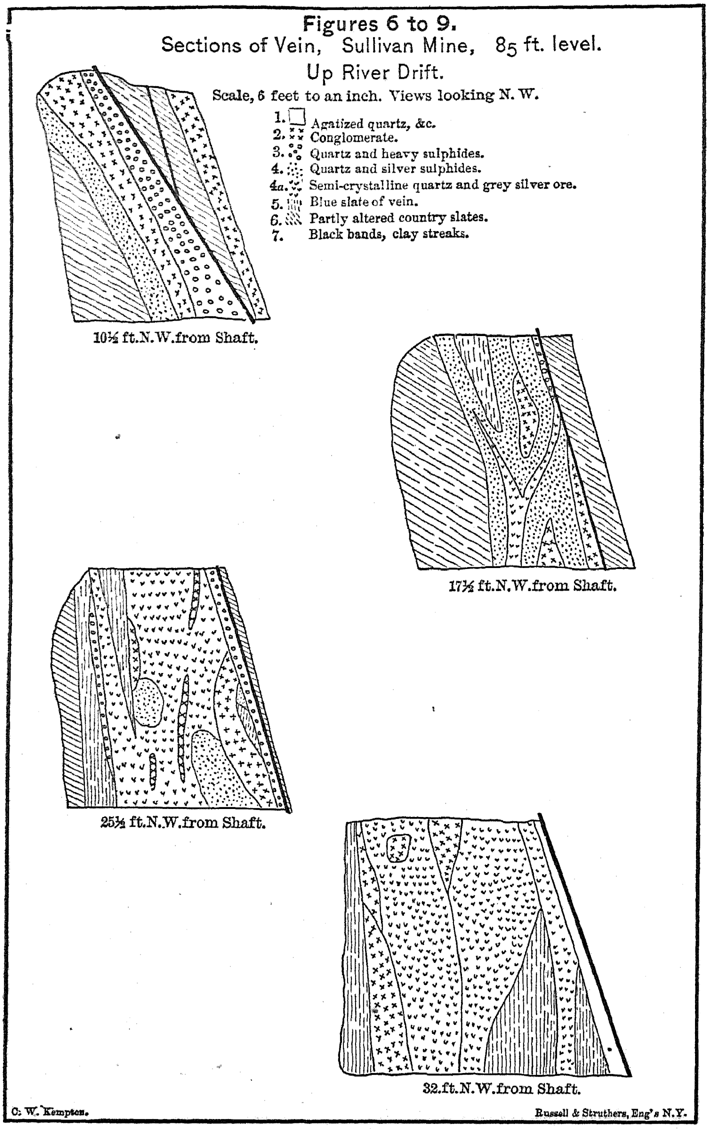
Of the silver minerals, stromeyerite is most plentiful, stephanite next, argentite (silver glance), common, native silver in flakes very plenty, threads frequent, lumps occasional.



Figures 2 to 5.
Sections of Vein, Sullivan Mine, 85 ft. level.
Down River Drift.
Scale, 6 feet to an inch. Views looking S. E.

Ruby silver is exceedingly rare; antimonial silver has been found. The occasional yellow copper sulphide met with has a peculiar lustre and runs very rich in silver.

As stated above, the course of the vein is from northwest to southeast, with the strike of the slate running parallel to the line of contact of the granite. The vein is in the slate, dipping at an angle



Figures 6 to 9.
Sections of Vein, Sullivan Mine, 85 ft. level.
Up River Drift.

of 70° from the horizon, northeasterly, toward the granite, which it probably reaches in less than a thousand feet. The slate also dips toward the granite at this place about 37° from horizontal,

although at some other places not more than 12°. (See sketch, Fig. 10.)

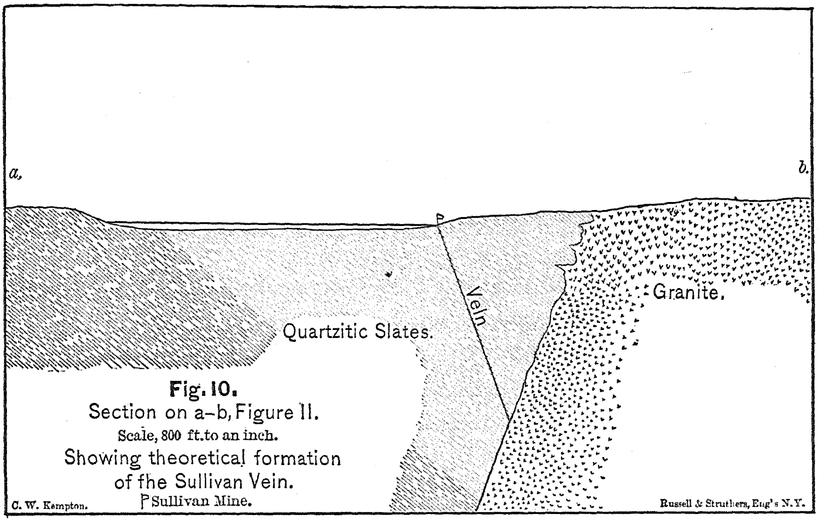


Fig. 10.
Section on a–b, Figure 11.
Scale, 800 ft. to an inch.
Showing theoretical formation of the Sullivan Vein.

At the contact of the slate and granite, the latter often penetrates the bedding of the slate, in known instances nearly 200 feet. The granite is much cut up by dikes of black trap, which also run into the slate, faulting the vein in several places. A sketch of dikes and faults is shown in the map. (Fig. 11).

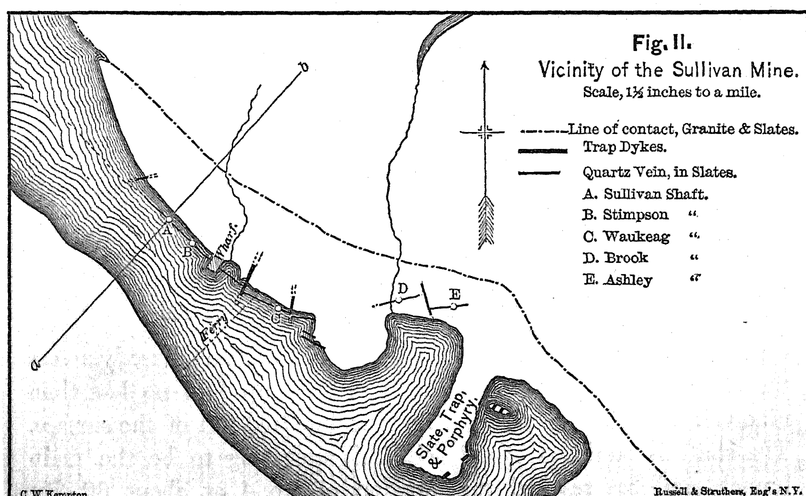


Fig. 11.
Vicinity of the Sullivan Mine.
Scale, 1½ inches to a mile.

The geological sequence of the formations appears to be, commencing with the oldest:

Quartzite slates.

Granite, and silver vein in slate.

Trap.

Quartz veins in granite and syenite.

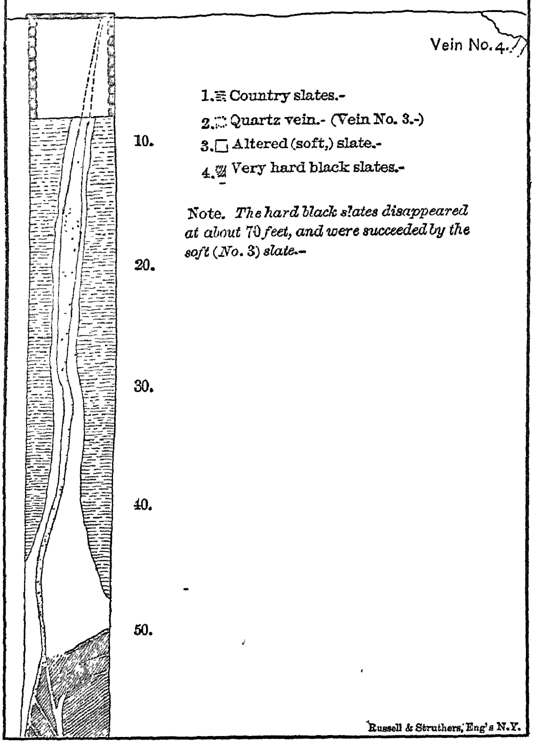
The general line of fissure of the Sullivan vein, passing northwest under the bay about three miles, strikes the shore at the Cline shaft, shown in section in Fig. 12. Here, owing to some local dis-

Fig. 12.

Cline Shaft, Hancock, Maine.

Scale, 16 ft. to an inch.

Section looking W. Showing junction of veins 3 & 4.

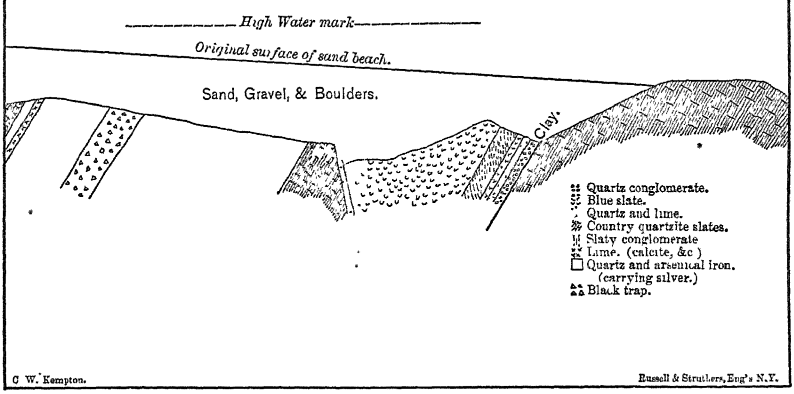


turbances, the vein is deflected more westerly and cuts directly across the slates, by which action it has been split up into no less than nine veins, dipping to meet within four hundred feet of the surface. The shaft is sunk on No. 3 vein, this appearing to be the main branch, and has reached the junction of No. 4 at about 60 feet from the surface, as shown.

The ore here is copper pyrites, galena, etc., at the surface assaying well for silver, and appears to carry gold and silver in form of tellurides at a depth of 60 feet. Average assays about $20 per ton.

Beturning to the Sullivan mine, and passing southeast a few hundred feet, we come to the Stimpson shaft, which has struck the vein, well developed, at a depth of 64 feet. About five hundred feet further southeast the vein is faulted by a large dike of black trap. Beyond this it continues in a direction more easterly, and is accompanied for a portion of its length in the next 700 feet, by a heavy quartzitic band, nearly 25 feet wide, carrying arsenical iron. This iron gives a show of gold, and in some cases gives an assay for silver. The vein proper is highly decomposed, and has not yet been fairly developed. The sketch (Fig. 13) gives a section across this formation, about 650 feet beyond the large dike, easterly.

Figure 13.
Formations at Waukeag Property,
Sullivan, Maine.
Scale, 16 feet to an inch. Section looking S. E.



The vein beyond this is faulted and deflected, and next worked at the Brook shaft, so called, about half a mile east, and also at the Ashley shaft, some hundreds of yards from the Brook. Fig. 14 gives an idea of the formations at these two shafts, their height being referred to high-tide level. In the Brook shaft some telluride ore has occurred, but not enough in quantity to enable the mineral to be identified. It carries both gold and silver. In the Ashley the same occurred, sparingly, and sulphides carrying $25 to $30 per ton in gold and silver have been found at the present bottom of shaft.

No further developments have been made on this vein and its

connections, but some nine miles southeast, in Gouldsborough, two veins have been struck in the syenite, which bid fair to become of value. "G" on the map (Fig. 1), the "Young" mining property, is a galena vein, with strike northwesterly and southeasterly, or about

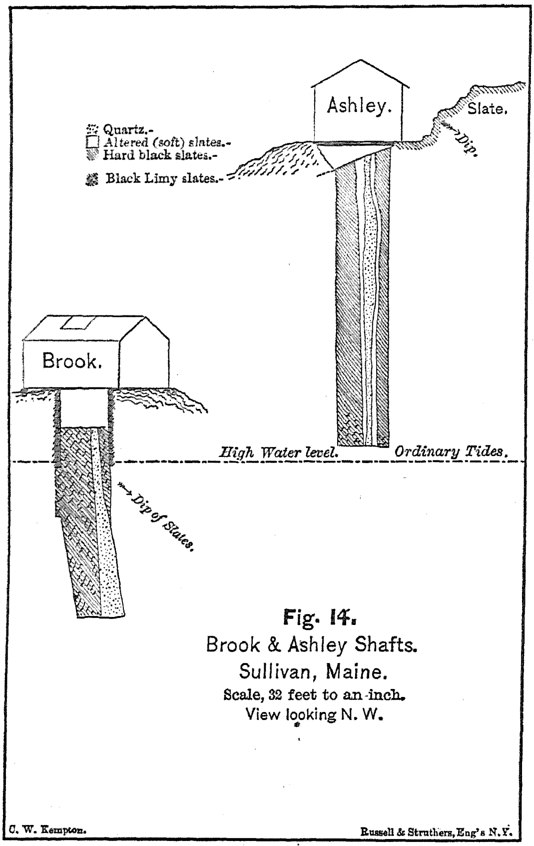


Fig. 14.
Brook & Ashley Shafts.
Sullivan, Maine.
Scale, 32 feet to an inch.
View looking N. W.

parallel with the Sullivan vein. "H" is made up of galena and copper sulphides, the surface ore assaying quite rich in silver. The strike is nearly north and south. Both these veins dip nearly perpendicularly.

Less than a year has sufficed to demonstrate the existence of strong fissure veins in this district, carrying high-grade ores, and to give every assurance of their permanency and value. As has been stated, the level surface of the country, covered with drift gravel, makes prospecting a slow matter, and there is no reason why there should not exist many mineral deposits in the Sullivan district as good or better than those already discovered.

DISCUSSION OF DR. CHARLES B. DUDLEY'S PAPERS ON STEEL RAILS, READ AT THE LAKE GEORGE MEETING, OCTOBER, 1877.*

REMARKS OF MR. ROBERT W. HUNT, GENERAL SUPERINTENDENT, ALBANY AND RENSSELAER IRON AND STEEL COMPANY., TROY N. Y.—In discussing Dr. Dudley's two most interesting papers, I feel a natural hesitancy in disagreeing with conclusions formed after such careful and conscientious work. But I think he has made his deductions from too meagre premises. The analyses of but twenty-five samples are entirely too few to justify the establishment of so important a formula. When we consider that he indirectly proposes to regulate an interest amounting, even at the present low prices, to over $20,000,000 per annum, we may well hesitate before accepting a formula based upon the chemical and physical analyses of but twenty-five steel rails.

Dr. Dudley touches the keynote in referring to the proposed labors of the Government Test Commission. They based their hopes of attaining success upon the chemical and physical analyses of a very large number of samples, and in the only branch of investigation which the short-sighted policy of Congress has allowed them to complete, *i.e.*, chain cables, we find that a dozen, twenty-five or one hundred tests gave no satisfactory results. It was only after many hundreds that they were enabled to draw conclusions.

There are so many circumstances bearing upon the life of a rail, that the task of extracting a correct result seems herculean. I presume I am right in stating that in our Northern latitudes early winter and spring months witness the breaking of the greatest number of rails, thus seeming to prove that an unstable road-bed has much to answer for. Then, again, broken and flattened wheels must be taken into account, but I am convinced that more rails have been broken by the treatment which they received before leaving the rolling-mill

* The remarks of the participants in this debate have been written out by the speakers, and are here given without any attempt to retain the order of the discussion. While the report does not, therefore, preserve the vigor and spontaneity of the debate, it is believed that it gains in value by giving the matured views of the speakers. The continuation of this discussion at the Pittsburgh Meeting, May, 1879 (beginning with remarks of Mr. Cloud on page 401), is appended for convenience of reference.—T. M. D., Sec'y.

than from any other cause. I allude to the injury inflicted upon them in the cold-straightening press, where each blow of the gag forms a wedge of particles of steel pressing upon surrounding ones, and thus serving to rend the rail asunder.

I am certain that of all the broken steel rails that I have seen, fully 75 per cent. have been ruptured at a gag-mark. So well recognized is this cause of breakage that the Troy Works and others have spent large sums in introducing machinery to more perfectly hot-straighten the rails, and thus leave less work for the cold-press. While I admit that Dr. Dudley's physical analyses show a difference between the broken and crushed and the unbroken and uncrushed rails, I am not prepared to accept these results as coming entirely from the chemical properties of the metal. If I mistake not, seven of the twenty-five samples are from crushed rails. May not these failures have been caused by mechanically imperfect bars, piped ingots, or some other mechanical defect? Then, again, the possibility of the steel having been overheated in the rail rolling-mill must not be ignored, for it is well known that the same steel worked at different temperatures will afterward yield widely differing physical results. We, who have to encounter the difficulties of manufacture, know how many and vexatious they are in their physical as well as chemical forms. Objecting to the assumption of a formula based upon such limited research, I propose to offer in lieu thereof a practice covering several years, under which many hundreds of thousands of rails have been manufactured. If the actual wear of these rails has been and is satisfactory, may we not assume that the formula of manufacture cannot be far wrong?

The life of the Bessemer process covers but a few years, and in that limited time wonderful changes have taken place. Only some six or seven years ago iron containing 20 per cent. manganese was called ferromanganese, and was difficult to obtain. It is now largely made with 74 to 84 per cent. manganese. In those days the ordinary spiegeleisen used in rail manufacture had but from 6 to 10 per cent. manganese, and was very high in phosphorus. It was generally accepted as a rule that rail steel must be soft; hence, while using this low-grade spiegel, the steel was low in both carbon and manganese, and when the carbon and manganese were increased the percentage of phosphorus also went up. The percentage of second quality or defective rails at this time was very large at all the works, and remained so until better and richer spiegel was obtained. Bessemer steel makers

were some time discovering it; but the fact is established that, as the percentage of phosphorus is increased so also must be the manganese, or the steel will work badly.

I do not believe that any maker can give Dr. Dudley's company steel based upon his formula at the price now ruling for rails. I know that a mill attempted to make five hundred tons for them according to it, and while doing so made over 10 per cent. of defective rails.

This production of second quality rails has an interest for the consumer as well as the producer; for if the steel is working so unsoundly, some rails must pass the closest inspection containing undiscovered flaws, which in the track will develop into "soft spots," "black spots," split, crushed, and broken rails.

I fear I made some of the steel which has given such bad results on the Pennsylvania Railroad, and so feel all the more at liberty to point to other rails which have and are giving far more satisfactory results.

The Troy Works, while the first to assume practical work in this country, had also the advantage of using for many years, in fact until late in 1874, the most approved brands of English hematite irons, and while using these irons as the base for their mixtures, were enabled to carefully experiment with American irons without the risk of unduly deteriorating the quality of their steel. Most of the other works in this country were compelled to commence with untried American irons. Both pig-iron makers and steel makers have had many a lesson to learn. I may be mistaken, but I will venture the opinion that at least six out of the twelve rails given by Dr. Dudley in his table as neither breaking nor crushing in service, were foreign rails. I refer to the use of English irons by the Troy Works not by way of drawing invidious comparison, but as a historical fact.

But in those early days they were afraid of hard steel, and as the color test for carbon was unknown, they were compelled to rely upon physical tests to judge of the steel's hardness. So I find soft steel was the rule at Troy up to 1871. In that year a different policy was adopted, and has since been continued. The formula for work on rail steel may be briefly stated to aim at .36 per cent. carbon; phosphorus and sulphur as low as possible, and only manganese enough to assure sound steel, the low phosphorus of the Lake Champlain irons making this attainable with a moderate percentage. By way of showing the practical results of the two policies, I propose presenting some data gathered from rails in use on the Boston and

Albany, New York Central, and Erie railroads. I select these roads as being trunk lines, whose tonnage and grades would be about the same as the Pennsylvania Central. I am unable to state the number of tons which have passed over the respective rails, knowing only the length of time they have been in the tracks.

The officers of several roads have kindly furnished me drillings, which have been analyzed by Dr. August Wendel, chemist to the Albany and Rensselaer Iron and Steel Company. Physical tests were impossible, as the rails are still in service, unbroken and uncrushed.

Of fourteen samples from one road, the average carbon was found to be .355 per cent., the rails having been made since 1871, and being now little worn after from five to six years' service. An official report from one road gives the percentage of breakage to the 2000 bars for one year as one broken Troy rail against two and two and a half rails of foreign makers, laid side by side. An official report of another road mentions Troy 1873 and 1874 rails as comparing favorably with any steel on the road, while of "G. T." rails, or rails made at Troy in 1868, and known to be of low carbon, the report says, "have seen eight broken rails in less than one mile on summit grade."

To the Boston and Albany road, 87,198 bars or 23,460 tons, have been furnished. Of these, 9917 bars were furnished before the carbon standard was increased, and of 4986 of them, or about one-half, a special record has been kept. They show a breakage in nine years of .28 per cent., while the other 82,212 bars, including the remaining 4931 bars of low-carbon steel, show a breakage of but .08 per cent., covering a life in the track of from one to eight years. Analyses were made from eight samples taken as drillings from some of the 1869 steel, which has shown the .28 per cent. breakage. The carbons were found to be: .21, .20, .19, .19, .22 .21, .22, .24 per cent. Two fuller analyses of the steel gave: carbon, .21; silicon, .022; phosphorus .116; manganese, .241; and carbon, .22; silicon, .037; phosphorus. .08; manganese, .250. This steel was all taken from the main track last season and put in sidings, owing to its being so much worn, most of it having been originally laid on curves. The carbon in six samples from rails made since 1871 ran from .30 to .39 per cent., having an average of .35 per cent. No rails furnished this road have been crushed. Accepting for a moment Dr. Dudley's formula for calculating what he calls "phosphorus units," these six samples gave an average of 40.9 units. Two rails were found which have been in the

track nearly five years and are now in perfect condition, that show an average of 52.2 phosphorus units, one of them containing .469 per cent. silicon. Let me here doubt the correctness of Dr. Dudley's statement: "As to silicon, the less the better," particularly as he continues: "Nevertheless, as by the Bessemer process it is impossible to make rails entirely free from silicon, a small limit must be allowed for it. As the process is ordinarily worked, perhaps .04 per cent. will not be too high a limit"—thus leading to the supposition that steel made by other processes would be certain to contain less of this element and that it is rank poison to rail steel. This is a position to which I must take most decided exception. I have devoted much time and thought to the effect of silicon on steel, and have become fully convinced that it is a desirable element, like many other good things, requiring to be used with judgment, and also, like some other blessings, frequently called upon to shoulder sins not its own. Certainly it is now well established that silicon gives soundness to steel in pouring, and without a sound casting the production of a perfect steel bar is a matter of serious difficulty. That Dr. Dudley is wrong in assuming it to have one-half the same effect as phosphorus "in rendering a steel hard and brittle" is proven, I think, by the following analyses: For a sample of hoe steel, which was perfectly satisfactory to the manufacturers and made by the crucible process, Dr. Wendel found silicon, .185 per cent.; in another sample, .158; in another, .150; in another, .249. In a fork steel he found silicon, .241; in another, .204. In Jenks's spring steel the silicon was .145; in another sample of the same brand the silicon gave .150. In Greeves's spring steel the silicon was .110. Other spring steels gave him silicon, .237, .252, .180, .182. In a steel used for machine screws the silicon was .223. In a sword steel used by a celebrated manufactory he found .258 per cent. silicon. A German scythe steel used by one of the best-known makers in this country,—a brand he has used for thirty years, considering it better than any other attainable, he found silicon .127. An axe steel gave .270. Park Bros. "black diamond" steel, which at the time the analyses were made was selling at 14½ cents, contained silicon .220, .204 and .216. In cutlery steels, .210, .255, .207, .147, and .229 per cent. silicon was found. Jessup's die steel had silicon .285. In a propeller casting made by Vicker's Sons & Co., and sent by them to this country as a sample of good metal for its purpose, there was .380 per cent. silicon. Some months ago, when discussing the possibilities of the Bessemer process with one of the

best known scientists, engineers, and crucible steelmakers of this country he differed with me as to some of my propositions, and taking me into his testing-room, picked out a piece of steel which had been experimented with, and, causing the test to be renewed, exhibited to me a most magnificent piece of metal, showing fine qualities both before and after tempering, saying in conclusion: "My dear friend, when you can produce such steel as that in your Bessemer converter, we crucible people will shut up shop." Of course I could only stand awe-stricken, and at last, with returning courage, beg a piece for analysis, which gave results as follows: carbon, .86; phosphorus, .016; manganese, .195; and silicon .306! If Dr. Dudley's phosphorus-unit theory is correct, the quality of the steel would have been the same had the analysis given silicon, .008; phosphorus, .153. I do not believe any steelmaker will indorse that supposition. An analysis of some Bessemer steel made in Rhenish Prussia gave carbon, .19; manganese, .47; phosphorus, .083; sulphur, .007; copper, .165; and silicon, .841. This steel, which according to Dr. Dudley would contain 66 phosphorus units, is represented as being very soft and welding with great ease—so much so that it was used with success as a welded steel head to an iron base for rails.

The analyses above given, with the exception of the Prussian soft Bessemer and of the propeller-blade steel, are of steels made by the crucible process, from samples collected with great pains from the consumers using them, and pronounced by them satisfactory for purposes where brittleness would be fatal. I am far from saying that silicon does not affect the tempering qualities of steel; but we do not temper rails. Neither do I advocate putting .469 per cent. of silicon in rail steel. Still, as already stated, there is a rail in service on one of the principal railroads of this country which has been in the main track for nearly five years, which has not crushed or broken, and shows but little wear, containing that percentage of silicon with .36 per cent. carbon, and .571 per cent. of manganese, to say nothing of its phosphorus, which happens to be .124 per cent.! If Dr. Dudley is right, that rail ought to have broken some four years and eight months ago.

For the reasons before given, I believe in putting enough manganese in the mixture to make certain of a sound working steel, and no more. But I know that a piece of steel test bar, containing phosphorus .10, carbon .35, and manganese 1 per cent., will bend around double, but exhibit much greater stiffness than had the manganese been less.

In treating more directly of Dr. Dudley's second paper, I must only

refer to much that I have already said: and alluding to Mr. W. H. Brown's report that "the rails of higher carbon are giving poorer wear than before the lower limit of carbon was raised," I must pass this over as not being supported by any authenticated analyses. For the fact of the great service of the crucible steel rails of presumably high carbon which were placed in the tracks of several roads many years ago remains, thus seeming to suggest some other reason for the poor wear of these supposed high carbon rails on the Pennsylvania Railroad.

As already said of Dr. Dudley's proposed formula for a perfect rail, so also of the analysis given by him in his paper of the rail containing carbon, .270; phosphorus, .10; silicon, .020; manganese, .259; sulphur, .051; and copper, .025, I can only say that such steel cannot be made by the ordinary Bessemer process without making unsound steel, and so endangering a large percentage of the rails in defiance of the closest inspection.

In conclusion, I must say that I have yet to be convinced that the Troy practice is wrong, and only trust Dr. Dudley will continue his researches, so that if he is right it may be proven by many analyses, and if wrong he may, as I know he will, say so.

As Mr. Shinn has stated for his company, I will say for the works which I represent, we also want the investigation to go on; but we do not want premature conclusions. If Dr. Dudley has been misunderstood, and has not intended to present his investigations as conclusions, he must blame himself, for his paper was presented to the Institute as being permitted "by the kindness of the officers of the Pennsylvania Railroad Company," it having been originally written as a report to that company; and in concluding the report he says: "I would, therefore, respectfully recommend that the following formula be prescribed for the chemical composition of rails for the use of the Pennsylvania Railroad, viz., etc." Hence it was perfectly natural for other railroad officials to accept that formula as the one approved by the Pennsylvania Railroad Company, and I believe that they as well as Dr. Dudley considered the whole question settled.

From this view of the case a Tennessee Railroad objected to some rails that a Pennsylvania mill was furnishing them, because, as they put it, "it is found that the proper formula is so and so," quoting the one given by Dr. Dudley.

In his paper I find this: "But in view of the liability to accident which a broken or crushed rail may occasion, I think no one will claim that a rail which has broken or crushed in service should

be classed among good rails, even though its tonnage may entitle it to be so rated."

But Dr. Dudley now objects to my making broken rails a prominent point in the discussion. If it is a false object of attack, he certainly presented it. Still if he now insists that it is crushed, and not broken rails which are so frequent on the Pennsylvania Railroad, I can only again refer to the report of the officers of the Boston and Albany Railroad, in which it is stated that of the many thousands of Troy rails which they have in their tracks *not one has crushed*, and only .09 per cent. broken. And if as he says many of the rails on the Pennsylvania road crush in the middle, while the ends remain intact, I must claim that it supports my theory of mechanical defects. If the chemical character of the steel alone caused it to crush in the middle of the rail, what kept that crushing from extending throughout its whole length? I must insist that no phosphorus-unit theory can be established on as limited an investigation as Dr. Dudley has yet made, particularly when I have presented in my paper fuller data which seem to disprove his deductions. But he has done a good thing in bringing about so much discussion as to what constitutes a good rail, and good fruit will yet be gathered.

He expresses himself as being gratified by the kindness with which he has been treated in this discussion. I do not think he should wonder at this, for my reading does not show me that when Mucius thrust his right hand into the glowing fire, a single one of his admiring countrymen spoke an unkind word to him.

W. R. Jones, Edgar Thomson Steel Works, Pittsburgh, Pa.—In this discussion I feel disposed to sustain Dr. Dudley in most of the theories he has advanced in his excellent paper on steel rails and their composition, and am free to say that his position in this matter has generally been misunderstood.

The Pennsylvania Railroad authorities having had in their track rails that gave excellent wearing results and rails that gave very poor results, naturally were anxious to ascertain the difference, chemically and physically, of the two classes of rails, and after carefully testing them both, Dr. Dudley presents a formula based on the results of his careful researches; and although the number of tests made are unfortunately too small to carry that conviction that a larger series of tests would do, I for one will admit that rails made according to Dr. Dudley's formula will be good rails, and the fact that miles of such rails are in the Pennsylvania Railroad track, and

have given and are giving the very best results, is proof conclusive of the correctness of this formula.

Dr. Dudley does not claim for his formula that the relation of certain proportions of carbon, silicon, and manganese to phosphorus is the only correct one, but in this specific case he uses the formula to show that rails made in accordance with it have given good results.

On first examination of the sketches showing the sections of the rails that did not give good results, I was led to believe that the cause of their failure was due to what is termed "split ends;" but after traversing over a mile of the road with Dr. Dudley, I discovered a large number of rails that were sound at the ends and very defective in the middle, and any number that were bad all over. So the assertion that has been made that Dr. Dudley has condemned rails simply on account of "split ends" will not hold.

From my experience, and I believe in this Dr. Dudley will agree with me, I think that putting manganese in the ratio of 5 to 1 of phosphorus is too low. I should think that placing manganese at about 7 or 8 to 1 of phosphorus would be nearer the mark, and I have made rails with a total of forty phosphorus units which, according to Dr. Dudley's formula, would indicate that the rails were too hard for safety, but they showed in the testing machine twenty-five per cent. elongation, and stood a remarkably good drop test, being subjected to a weight of 1600 pounds falling 24 feet, resting on bearings 36 inches from centre to centre without sign of fracture, showing that the material was tough and strong.

Eminent French metallurgists place silicon as $3\frac{1}{3}$ to 1 of phosphorus, so that while Dr. Dudley's formula may be applicable to one class of steel, yet I believe and know that steel rails containing as high as .20 per cent. of phosphorus can be made that will give the very best results.

Colonel De Funiak, engineer of the Louisville and Nashville Road, tells me that the high phosphorus rails from Terrenoire, France, in use on their road, have given the very best results, and so well was he pleased with these rails that he seriously thought of having American rails made in accordance with the Terrenoire formula, viz.: high phosphorus, high manganese, and low carbon. Manganese is no doubt the corrective agent of an excess of phosphorus and sulphur, and is as necessary to the steel-maker as calomel to the old-school physician.

Now my friend, Mr. R. W. Hunt, who made a great many of those bad rails that the Pennsylvania Company complain of, will

admit with the light thrown on steel making since they were made, that by lowering the carbon and raising the manganese fully fifty per cent., these rails would have given far better results.

What I wish to particularly impress on the members of the Institute, is that there is a well-defined formula by which steel can be made containing as high as .20 per cent. of phosphorus, with proper proportions of manganese and carbon, that will give as good results in wear and safety as those made in accordance with Dr. Dudley's formula. I will say this from my experience in steelmaking, that were I located west of the Ohio, where high-phosphorus iron is used, I would not purchase a pound of ordinary spiegel, but would use ferromanganese in order to make a safe and durable rail. That this formula is not as well known to the rail-steel maker as it should be, is due to the selfish and conservative character of the steelmakers themselves. Each one, after developing a new and correct theory and practice of steelmaking, locks the secret in his breast for fear he may never be able to get another. A full and open discussion of this matter among the steelmakers would develop a large amount of concealed information; but unfortunately there is no such disposition existing.

I cannot agree with Dr. Dudley in considering the tests made on the Thurston testing machine as final in regard to the character of the steel. The Thurston torsion machine, like all machines in existence (excepting the Government machine at Watertown Arsenal, which Mr. Holley described so graphically last evening), is only approximately correct, and can only be used as an adjunct to other tests. I will read the tests, made under Dr. Dudley's supervision, of two pieces of steel, each 1 inch square, which represents only ¼ inch in length of a 14-inch ingot. One piece of this steel was rolled at the highest heat attainable without burning or injury, and possessed an ultimate tensile strength of 70,650 pounds per square inch, with an elongation of 37½ per cent. The other specimen was rolled at a dull red heat, and had an ultimate tensile strength of 87,007 pounds, with an elongation of 19.8 per cent. This shows that steel of the same chemical composition may, by difference in treatment in rolling, give widely different results in the torsion machine when it has not been injured by either heating or rolling. This variation is likely to occur in every mill in the country.

The heating of the steel varies in accordance with the character of the machinery used in rolling. You take a weak train of rolls

and inadequate power, or, as is the case in some mills, have too few passes with excessive reductions for speed in rolling, and you will find the heating carried to the extreme limit of safety, with very often a burnt rail that inevitably shows up bad in the track. Yet this class of mills will show that their steel has low tensile strength and high elongation, and yet the steel may be inferior in chemical qualities to that of another mill that has ample power and strength in every respect with proper reductions, and where no fears exist of trains being broken or wrecked. Here the steel is rolled at a moderate heat, and the finished rail has the greater density, and yet it may be rejected by reason of its having greater tensile strength and less elongation.

The mills that roll rails that give the least tensile strength and greatest elongation are more apt to furnish the greatest amount of bad rails. Rolling at a high heat they close defects that at a moderate heat would be developed, and the result is a defective rail is made and accepted simply on account of its percentage of elongation being greater than another rail of the same chemical composition, heated and rolled at a proper heat, and the defects developed and cut out; yet the Thurston torsion machine shows it lacks in elongation. You condemn the good rail and accept the bad one. Now take the two rails to a drop-test, and you find that the rail with the good elongation breaks under a 24-foot drop of a 1600 pound weight, while the rail that has been rejected will astonish you by standing the test. This is no supposed case but is an actual fact, and from my experience I accept no one physical test. I wish it to be distinctly understood that I do not condemn the Thurston torsion machine, on the contrary, I deem it of great value as an adjunct to other tests, and will say this in its favor, that no steel mill in the country should be without one; but what I want to say is that the Thurston machine alone would not be the proper thing to determine whether this or that rail was good or bad. I can say that at our works we use the machine constantly, and frequently find that after other tests have been made this machine will develop some characteristics of the steel that other tests failed to detect, and lead us to make further tests of steel which would otherwise be condemned for the purpose for which it was designed. At other times this machine has indicated that the steel was good when other tests have demonstrated that it was bad. Dr. Dudley, I believe, will agree with me on this point, that it is not advisable to rely on one particular test alone, but it is possible with

but a slight increase in cost to have a combination of tests that will insure as far as practicable all that he may desire.

I may add as conclusive proof of the correctness of my views in this matter that our works have furnished to a prominent road fully 16,000 rails, which have been in use over three years, and of this number only two rails have been broken, and those under such exceptional circumstances as to free us from all responsibility. And, generally, our record as to broken rails is unexceptionably good, and we have strong hopes that time will show our rails are also equal in wearing qualities to any made.

Regarding the cause of broken rails as mentioned by Mr. Hunt, of Troy, I agree with him that 90 per cent. of the rails that break in the track were cracked or strained in the mill under the straightening press; of course, I mean rails that have a fair chemical composition. I believe I was the first to condemn the old method of hot-straightening, which was to run the rails, after being sawed off, on to what was termed the straightening plate. Then two workmen seized it with malice aforethought, raising each end about 24 inches high, and let it fall on the plate. This procedure I watched carefully for over two months, and felt satisfied that instead of doing the rail any good it was doing it a positive injury, for by raising the ends of rails the under side of head and flange was stretched, and at the cold press I invariably found that every rail had to be gagged repeatedly at this point, and undo what the two men had done on the straightening plate.

Most of the leading mills have discarded this barbarous method and have good modern improvements in its place, and this objection is now removed. In cold-straightening we straighten rails at a temperature so high that not a member of the Institute could keep his hands on them but a few seconds, unless his hands were very callous.

Too many mills have their presses working under the principle of a shear rather than that of a press, and I have seen presses so arranged that do greater damage to the rails than any load or shock that they are liable to in service. Generally speaking, great progress has been made in details of furnishing rails over the earlier practice, which did so much injury to rails before they left the mill.

A word in regard to Dr. Dudley's formula of phosphorus units. Since the doctor proposed this formula how many of those who condemmed it as being egregiously wrong, had any idea whatever of the relations of carbon, silicon and manganese to phosphorus? Although the doctor may be wrong, and I believe he is, yet he was the

first to endeavor to establish a formula of this kind, and is, therefore, entitled to the thanks of steelmakers; for although it may not be correct, it is much nearer the mark than what others have simply guessed at; and the direct results of his investigations have been to stimulate investigations by others and throw much light on a dark subject.

A word in regard to softness and hardness of steel rails. Railroad engineers have differed much on this point. In making rails for the New York Central Railroad the inspecting engineer was afraid that our steel was too soft, and asked me to increase the hardness. I told him that the steel was as hard as consistent with safety; yet, at his request, I increased the hardness by adding carbon until the steel would temper, telling him that we would not be responsible for rails breaking. I was reminded of an old furnace friend of mine who for over twenty years had been making miserable iron called mill or forge iron, and had finally begun to make Bessemer iron. Meeting him one day he told me that he was making a No. 1 Bessemer iron, and I asked him how his iron stood in regard to phosphorus. He answered, "Well, it aint got quite as much as we would like to have, but *I can put more in it.*" If steel rails are too hard, the railroad authorities are responsible for it, for they have always been anxious to get them high in carbon, not realizing that other hardening elements existed. And I would say here that the majority of inspectors sent by railroad companies to inspect their rails generally haven't the minutest idea of what a good rail should be. It is very amusing to watch them looking over the rails and rejecting those that they say are not straight, when they have not a tracklayer in their employ who can lay and spike a rail as near a true alignment as any rail they have rejected.

In order to show that the steelmakers can make steel with almost any desired elongation I present tests of special steel made for stamping purposes by our company.

No. test.	Tensile strength, at elastic limit.	Tensile strength, ultimate.	Elongation.
1	39,762	53,298	107.81 per cent.
2	37,751	55,865	100.68 per cent.

This steel certainly should be soft enough to satisfy the most fastidious engineer that believes that soft steel outwears hard steel. We have also made steel that has stood a test of 170,000 pounds per square inch, so here is certainly range enough to satisfy any one.

In regard to the two rails shown by Professor Egleston illustrating

the flow of metal, I would ask the Professor if he examined the condition of the driving and truck wheels, as the deformed rail would seem to show that the tires and wheels were badly worn, tending to cause the metal to flow over the sides of the rails. Both rails, however, show that they were too soft and inadequate in strength to withstand the loads passing over them. I am led to make this inquiry of Professor Egleston by the fact that in 1859 and 1860 the Pennsylvania Railroad had made for their steep mountain grades an 84-pound rail, which was wider in the head than the ordinary 64-pound rail on the rest of the road. The 84-pound rails were, however, removed and replaced by the 64-pound rails, since the former did poor service, owing to the fact that the wheels being worn to suit the 64-pound rail, they crushed the sides of the heads of the 84-pound rail.

(In reply to Mr. Wm. Kent.) It will not do to rely alone on the Thurston machine as the deciding test of what is a good or bad rail; for instance: We will take a blow or heat of steel that will furnish say twenty-four rail blooms, we will heat and roll twelve at the highest heat attainable, and heat and roll the balance at a low temperature.

The first twelve rails may show a percentage of elongation of say twenty-two per cent., while the other twelve rails rolled at a fair heat, but colder, and having greater density, will not show an elongation of say sixteen per cent.; yet according to your view the first twelve rails that were heated the highest and probably slightly burnt, and would certainly show the poorest wear in the track, would be accepted simply because a piece one inch square cut out and tested in torsion machine showed less tensile strength and greater elongation than the other twelve rails heated and rolled at a fair heat without any possible danger of injuring the steel. These latter have a greater density, and by reason of being finished colder there is necessarily less displacement in molecular arrangement. You would reject the twelve rails that were the best, and accept the twelve rails that were the worst, besides doing Professor Thurston's machine a positive injury by relying on a single test. And this is the particular point on which I object to Dr. Dudley's prescribing the test by cutting from the head of the rail a test-bar one inch square, turning the centre five-eighths inch in diameter and one inch long. Now we see clearly that the first twelve rails I have spoken of will give a diagram nearer the test prescribed, and yet every one may be slightly burnt, while the other twelve that are free from such danger and rolled at what every

steel man knows to be a safer and better heat, will not meet the test. If you give the torsion machine a proper chance it will prove that the first twelve rails that you thought the best are really the poorest rails. Cut out specimen bars from the two lots, anneal them both, and the machine will tell you if the first twelve have been burnt so as to injure them, and it will also tell you that the twelve rails rejected were far better rails than they were supposed to be.

PROFESSOR THOMAS EGLESTON, School of Mines, Columbia College, New York City.—While admiring the ability with which Dr. Dudley has worked out the results in his paper, I must say that I think he has drawn his conclusions from altogether too few examples, and that they therefore seem hardly to be warranted. He has given us the analyses of twenty-five rails under very nearly the same conditions, many of which were rails that were bad. We should think it a very unfair way if we should pick out such a large percentage of bad people in a community and then judge the whole population by them.

I have been engaged in researches of this character since 1873. I must confess that I do not yet feel willing to draw such positive conclusions. The more I investigate and study the more clear does it seem to me that rails are affected by such a large number of conditions, many of which have not been studied at all, that we should be careful about applying the conclusions drawn from one set of conditions to cases where the conditions are either quite different or not exactly the same. I do not feel certain either that Dr. Dudley has given the proper positive or relative values to the coefficients by which he determines the influence of his "hardeners."

Leaving this, however, entirely out of view I think this whole question of rail research is just now being investigated in a false direction. We are in fact asserting that the chemist can solve the whole problem, which I am far from believing. I have had occasion within the last eighteen months to ask myself a great many times, what is the property which is expressed by the word hardness? and since the month of October, what does Dr. Dudley mean when he prescribes the limits of his hardeners? If we ask any physicist what is hardness, he will tell us that it is the power of resisting abrasion. Now we all know of substances which we call hard, which will not resist abrasion at all. I have had instances cited to me of rails which have borne 80,000,000 of tons traffic in the track, which were abraded at a rate of about one millimeter for 20,000,000

tons, which suddenly, without any apparent cause, gave way and dropped to pieces. The property to resist abrasion of iron and steel, under conditions quite different however from those of the permanent way of a railroad, is very often synonymous with brittleness; but when we say hardness we do not mean brittleness, we mean something that shall resist wear and be tough at the same time. We refer to scales of hardness and of temper as if they were positive and always had the same values, and yet some of these substances to which we fix a number on the scale with great certainty may vary as one to four. For commercial purposes this variation may in some cases not have much value, but in scientific research it is always of the greatest importance. I have been studying this subject carefully for some time, and will venture to assert that no two rails which have the same amount of phosphorus units or "hardeners" which Dr. Dudley has presented to us have the same hardness.

I have ascertained by direct experiment that a physical change goes on in the rail from the cold-rolling which it undergoes, which makes irons and steels of the same initial physical and chemical composition no longer suitable for the same services. It will be remembered that some years ago I presented the results of some experiments made during eighteen months, and of others collected over a number of years in France and elsewhere in Europe, from which some very curious conclusions were drawn. Among these it was shown that the curve representing the fracture of rails during a given period commenced from the day in which they were laid, and continued in an ascending scale up to about eighteen months and then suddenly fell to nothing, and that the line then remained very nearly horizontal. Breakages after this time occurred only from causes which would always produce them. I also showed that any flaw existing in a rail when it was laid down, even though it was imperceptible, would sooner or later cause the fracture of the rail.

I have the pleasure of exhibiting to the Institute two rails, one of which is still good, and has borne 80,000,000 tons traffic, while the other is worn out, and has had only 6,000,000 tons. Both of these rails weighed fifty-two pounds to the yard. The first was taken from the track at Spuyten Duyvil, on the New York Central Railroad, and has been eleven years in service. The other, from the curve leading into the freight depot in Hudson Street, New York, has been in use but six years. The first is good for 80,000,000 more tons traffic, the other is useless under any circumstances.

The analyses of these rails are as follows:

	No. 1.	No. 2.
Phosphorus	.0043	.0726
Manganese	.3962	.2918
Sulphur	.0049	.0091
Silicon	.0196	.0392
Carbon	.2850	.2032

Fig. No. 1 shows the wear of the rail No. 1; Fig. No. 2 that of No. 2; and Fig. No. 3 the head of No. 2 as it would appear if the

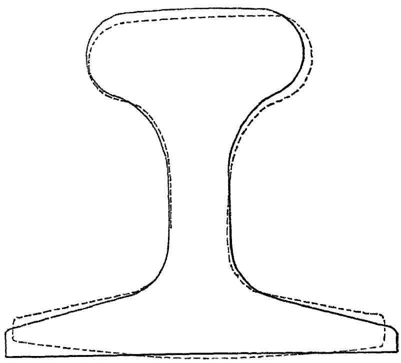


Fig. 1.

sides were rolled back on the rail head, and shows the part actually worn off from the head.

It will be seen that both of these rails have been thoroughly cold-rolled, and that they have undergone a serious physical change in the track. Now when we consider that the ordinary engine weighs from 20 to 45 tons, and that its bearing surface on the rail is about three-quarters square inch, we must admit that this weight distributed over so small a surface at every instant of time must produce some change. If you will notice the first specimen, which is superposed on its template, you will see that there has been here a cold flow of metal, which has thickened the flange and the web of the rail a little, while the upper part has been abraded about two millimeters.

In the second specimen the rail has been abraded and worn down, as is shown by the fact that what is now the inside of the rail was formerly its outside, which still shows the imperfections of the rolls, and that as it has worn down into the flange, the latter has divi-

ded itself on the two sides. It will also be noticed that the foot and web of the rail have had a cold flow of metal through them,

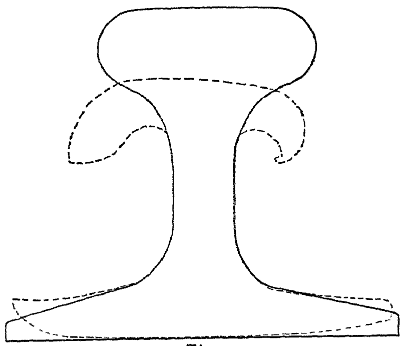


Fig. 2.

so that they no longer correspond to the original template of the rail. These rails if judged by the phosphorus units are not essen-

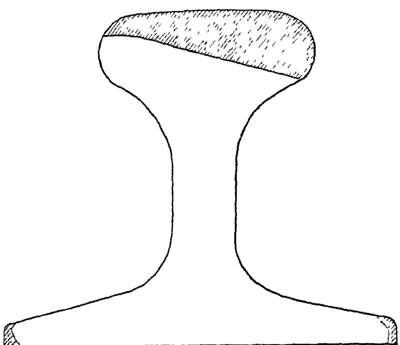


Fig. 3.

tially different, yet they are essentially dissimilar in regard to their resistance to abrasion. It is time that the chemist surrendered his complete and entire control of this question of rails, and railway corporations should turn their attention to physical and mechanical tests. It has been my habit to advise making these examinations by taking the test-pieces for torsional and tensile strain out of all parts of the rail, and not out of one, and to carefully examine the structure of the test-pieces, as well as part of the rail not mechanically tested.

The experiments which I have made prove conclusively that the temperatures at which rolling commences and at which the rail leaves the rolls and is laid upon the straightening bed and allowed to cool,

have a very decided influence on its life. I have been able to show that at times the condition of the steel in the centre was not at all the condition of that on the outside. I have also been able to show that the bubbles which were originally in the ingot were sometimes oxidized on their interior surfaces sufficiently to prevent welding. I have seen them following the contour lines of the rail section, and have sometimes noticed a concentration of impurities at these points, and sometimes near the centre of the rail. Whenever these bubbles come to the outside, there is the commencement of a flaw, and no matter what the phosphorus units in such a rail are its life will be shortened below that which would otherwise be its normal resistance to traffic. In some recent examinations I have seen these bubbles take the shape of the contour of the rail with such regularity and in such numbers that it was difficult at first to believe that the rail was made of ingot steel, so closely did its structure resemble that of weld iron. In a recent communication to the New York Academy of Sciences,* I have cited some instances of irons perfect in their chemical composition which were worthless on account of physical defects. At the St. Louis meeting of the Institute in 1874,† I referred to some steel rails made at Creusôt, which were broken in the tracks of the Northern Railway of France. These rails stood all the chemical and mechanical tests required by the contract and should have been good rails. From physical tests made since the Lake George meeting of the Institute on the pieces of the rails which I have carefully preserved, I have shown that these rails were very defective at the time they were laid in the tracks. Here, then, are irons chemically good, and steels mechanically good, and yet all are worthless. We certainly could not draw any conclusion from their phosphorus units.

I have recently been investigating the effects of sudden blows, like that of the engine, on a rail which is either too high or too low. The rail is thus subject at one time to abrasion and to slow compression, relieved at every instant while the train is passing, and to abrasion and sudden compression when the train falls from the high to the low rail. That these causes do produce an effect on the physical structure of the rail I have succeeded in making visible.

I have recently seen two hooks, one of which had frequently raised stones of 12 tons weight on the Brooklyn Bridge, which broke with a weight of 6 tons; the other, after long service, was pulled apart by men hauling on a rope passing through the block. The change in

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* *Science News*, 1879, p. 71.

† Transactions of the Institute, vol. iii, pp. 62, 63.

the physical condition of the first hook on the small surface which had been compressed was quite visible to the naked eye. My researches, however, have not gone far enough for me to reach any definite conclusion as to the effect of compression on the life of a rail.

I am making plans for an instrument with which to study all these phenomena; but as, up to the present time, my researches have been made wholly at my own expense, I have not been able to go very rapidly. It has been very difficult for me to get the material to work with, as I have not yet been able even to interest the great railroad corporations in these researches.

The condition of the road-bed and also of the wheels and tires have a decided influence on the wear of rails. If, for instance, the tire is softer than the rail over which it goes, it will be more likely to wear out the rail than if it were harder. I have had occasion to show that a soft tire will wear out the hardest rail in a comparatively short time.

Such researches as these are difficult to make. There are so many unknown quantities in the equation to start with, that the solution seems almost hopeless; and there often is a great temptation to eliminate some of them with facts drawn from insufficient data. Not only is the research difficult in itself, but we may have all the facts in our hands, and yet they may be useless to us, since we may not be able to translate them, as it often takes more time to interpret the facts than to obtain them.

Too little attention has been paid by both manufacturer and engineer to the ultimate composition of rails. We are satisfied generally in looking for carbon, phosphorus, sulphur, manganese, and silicon, without looking for the other ingredients. We know that copper is present in irons and steels made in some districts in Pennsylvania, and its presence may be more common than we have supposed. Mr. Gruner, of Paris cited the fact some years ago that the presence of aluminium in certain ores in France prevented the irons made from them from being used for crucible steel. We have learned only within comparitively few years that tungsten, chromium, and other substances may take almost entirely the place of the carbon, and yet the material be steel; and that they may be present in considerable quantities even in the presence of carbon. Calcium and magnesium have also been found in small quantities. I have recently made some analyses of Bessemer ores containing 7 to 8 per cent. of titanium, the presence of which was not even suspected by the parties using them. Berthier cites some analyses of cast iron

which he made, in which there was only 80 per cent. of iron. The amount of foreign substances other than carbon present, was nearly 16 per cent. He also cites some Spanish irons containing several per cent. of arsenic. These substances, which are in the ore, must have some effect on the product.

In fact I think we need an entire revision of the subject, and that our great corporations should undertake, not the chemical examination of irons only, but their physical and mechanical examination as well.

In conclusion I beg to say, however much I may disagree with Dr. Dudley's conclusions I shall always admire his methods of research, and the manliness with which, in the interest of science, he has announced them and invited criticism.

MR. A. L. HOLLEY, New York city.—A year or two ago I would have undertaken (for a suitable retainer) to convey more information on this subject than I am prepared to disseminate now. As soon as we had begun to get correct analyses, and in a general way to observe the more pronounced effects of the "hardeners," some of us, at least, rather jumped to the conclusion that we could predict all the important mechanical results of chemical composition. A large number of physical and chemical comparisons, made especially well at Terrenoire, and others at the Norway Iron Works in Boston, on phosphoric steels, also some experiments on rather pure steels in Sweden, seemed to confirm this expectation.

But as increased experimenting went on, it began to be suspected that testing-machine results might not after all foreshadow other physical qualities, such as resistance to abrasion. Meanwhile the United States Test Board had been making some 400 mechanical and some 100 chemical tests of steels; several experts had undertaken to get from them, a law of strength, and to lay down a rule of synthesis for steel for various uses. But the more they tried to unravel the relations between the chemists' and the testers' tables, the more they came to the conclusion that these experiments were of inestimable value—in revealing the importance of more. Two thousand machine experiments have been necessary to enable Commander Beardslee to establish his important working conclusions about chain-cable irons.

During the Paris Exposition, the Terrenoire engineers, who are most indefatigable and painstaking experimenters, published some elaborate tables of results on steels of varying carbon, and nearly alike in other elements; and other results on manganese steels and on sili-

con steel castings. But such experiments, however important,—and they are of extreme importance in directing the course of other experiments,—do not *yet* give us the two great practical every day facts we want, viz:

1st. The reactions of carbon and the metalloids *on each other*, under even the most simple stresses.

2d. The more remote fact—the effect of complex structure on resistance to complex stresses, such as impact, abrasion, and transverse strain on rails.

The paper of Dr. Dudley, and the intelligent discussion which it has called out, have given a new and increased impetus and direction to such investigations; the result of which will, of course, be the more rapid attainment of working conclusions in regard to the composition of rails and structural steels.

But we are not entirely in the dark; we have some evidences of the duration of rails of certain compositions, which do not point exactly to Dr. Dudley's conclusions—evidences which, although not embracing all the elements of correct conclusions, possess perhaps as many such elements as Dr. Dudley's evidences do. Those put forward by Mr. Hunt, of Troy, and by Dr. Egleston, are important among them.

When, in 1863 and 1864, I was studying the Bessemer process in Sheffield, I became quite familiar with the manufacture and qualities of the product at Bessemer's, Brown's and Cammell's works. The Cumberland irons used were all about alike, the blowing was all alike, and the steel was all alike to this extent—it had less ductility than rail steel now has. We had no trustworthy quantitative laboratory analyses then, but we had some qualitative if not quantitative analyses of carbon and silicon on a large scale—the vessels were uniformly and invariably *turned down "young."* So early were the heats finished, that the men from other countries, studying the process, had great difficulty in learning the incipient change in the flame which determined the finish. It is therefore certain, that the steel produced all these years was high in carbon and silicon; how high we can nearly tell by blowing the same irons in the same way. Now this is the very steel which made the early English rails which gave such remarkable wear. These were the rails which on the Chalk Farm and at Crewe Station, on the London and Northwestern Railway, wore out twenty-five to thirty iron rails in repeated experiments—for a good iron rail would last but about a week in some parts of Crewe Station. We are not now considering breakage, but the effect of carbon and silicon on wear.

On the Great Northern Railway of France, the Terrenoire phosphoric rails, containing about a quarter of a per cent. and more of phosphorus, and nearly one per cent. of manganese, but only about a tenth of carbon, have been down at least six years. The best evidence that they have been satisfactory is that the Terrenoire company is still making similar rails for this railway, and although the rails have now less phosphorus and more carbon, the former still exceeds the latter element. As to silicon, the repeated and authenticated results of the Terrenoire steel castings, which have a quarter per cent. and more of silicon, although they are especially free from *silica*, prove at least that silicon, in proportions considered quite inadmissible here, does not harm steel castings. A 12-inch shell of this steel goes through a 12-inch armor plate without other deformation than a small upsetting, and cannon of this steel stand more deformation without cracking than the hammered steel cannon they were competitively tried with.

Thus it appears that certain desirable qualities may exist in steels which have high percentages of various metalloids. These facts are not indeed complete; all the questions that could be asked about these steels cannot be answered; but these facts are drawn from large experience and many sources. It should seem strange if the ingredients of good rails cannot be varied largely beyond the specifications laid down by Dr. Dudley.

We are bound to admit, however, that his impressions in favor of low carbon, are in a certain degree confirmed by some experiments made in 1874–5 by Mr. J. T. Smith, manager of the Barrow Works, on some rails that had lain for eight years in the Furness Railway. Twenty rails which ran in carbon from 0.28 to 0.32, and averaged about 0.30, showed 13.54 per cent. wear, while ten rails which ran in carbon from 0.36 to 0.50, and averaged 0.42, showed 15.18 per cent. wear.

The apparently contradictory results which this discussion has elicited, should not, however, be deemed contradictory. They are the outlying and detached phenomena which rather prove the existence of harmonious and discoverable laws, although the complementary phenomena still lie in the great unknown.

Mr. Wm. Metcalf, Pittsburgh.—I would call attention to the question of hardness, as partially developed by Prof. Jno. W. Langley, in another way, which seems to be particularly appropriate to the subject of the endurance of rails. Prof. Langley having found the old methods of punching and scratching to be of very little

value as means of determining any useful property of a metal, developed the following plan. He used a simple contrivance in which a diamond drill was made to act vertically on the specimen of steel. This drill was fastened above to a lever beam, which could be weighted. On the shaft of the drill was a pulley (A), by means of which the drill could be revolved. He decided that "the accurate determination of hardness as a function of abrasion is indispensable. Now if we assume the proposition that the proper measure of hardness is the quantity of material abraded under the action of a constant force (and what other exact definition can we have?), then if the weight (W), is maintained constant and a given number (n) of revolutions given to a drill, the work done, calling a the intensity of force lying along a tangent to the circumference of A, will be $2 \pi r W n a$. The mechanical result will be a certain amount of drillings which can be most accurately determined by weight, and the hardness, H, will be

$$H = \frac{2 \pi r W n a}{\text{weight of drillings.}}$$

"Of course a is variable, because, as the drill sinks deeper it will take a wider cut, but the average value of a can be found by winding a string around A and then applying the moving force through a spring balance, whose indications are read for every revolution, these readings would be summed and divided by n to give the mean value."

By means of this apparatus the following experiments have been made, which, although they are too few to develop any law, are enough to show that valuable results may be obtained in this way.

It was desired to obtain the best steel for wire-drawing dies, where resistance to abrasive wear is the one desirable property, as it also seems to be in rails.

Four samples of steel were tried of the following composition: The first was a sample of high tool steel; the manganese was not determined, but is believed to be very nearly the amount given in the table. Its abrasive resistance was taken at 100 to afford a basis of comparison.

No.	C.	Si.	P.	S.	Mn.	Tungsten.	Abrasive Resistance.
1.	1 079	.039	.044	.004	.100		100
2.	1.370	not det.	not det.	not det.	not det.	.78	74
3.	2.370	"	"	"	.180		1520
4.	1 860	"	"	"	.566		223
5.	2.890	.140	.020	.031	.260		Not determined.

No. 5 is the analysis of perfectly satisfactory imported dies.

Nos. 2, 3, and 4 were not examined except for manganese, carbon and tungsten, because it was supposed that either manganese or tungsten was the great hardening and resisting element.

If the above experiments indicate anything it is that carbon comes prominently forward once more as the one useful element in steel.

With such abnormally high carbon as 2.37 and 2.89, manganese comes in as the cementing element which enables such steel to be worked, and so far as our experience goes, it is the only element which does not impair the qualities of very high carbon steel.

Other metalloids or metals should be looked after. An iron containing silicon .045, phosphorus .022, copper .145 yielded a magnificent homogeneous steel with carbon at .4 per cent., but when it was made into steel with carbon 1 per cent. it proved perfectly worthless. In making experiments for the Test Board, we observed very decidedly that the tendency of phosphorus was to make the steel very frothy in the crucible and unsound in the ingot, and when we reached phosphorus .5 per cent., the steel nearly boiled out of the pot, and the ingot was very like a sponge.

Mr. Dolphus Torrey, Philadelphia.—It seems to me that the conclusions reached have been drawn from insufficient data, as the influences of the treatment which the steel receives in the process of manufacture and the subsequent handling of the rails are practically ignored. In fact they cannot be sufficiently considered in the way in which the subject is approached. If any attempt should be made to keep a record of such things it would be either lost or misunderstood by the time the rail might be by accident selected for testing, owing to the changes of officers by railroad companies and of employés by steel companies. Instead of testing rails accidently selected, the history of the making of which cannot be ascertained, I suggest that the proper way will be to make rails for the express purpose of being tested, so as to be able to define the influence of various chemical mixtures or solutions in the steel and of various methods (and degrees) of heating and manipulating it. I would do this in the following manner: Cast from one charge in the converter two ingots of steel, one compressed, one not. Reduce each of these to four blooms. The blooms of each ingot should be rolled into rails, after being brought to different temperatures,—say high, very high, moderately high, moderately low, and very low heats,—thus giving from the one mixture of steel eight qualities of rails. Cutting these rails in two and annealing one-half of each I would have sixteen

rails, all from the same charge, and differing in qualities of endurance for service in a railroad track by unknown quantities, perhaps no one knowing which will best resist abrasion. I would accurately and carefully curve these sixteen rails and lay with them a small circle of track (say about sixty feet in circumference). Then making a circular truck, mounted on a considerable number of wheels (say sixteen to twenty) and loaded to correspond to the weight per wheel customarily carried in actual railroad practice, I would cause the truck to be rotated rapidly. Any one investigating the possibilities of the case will be greatly surprised at the short time taken to give a tonnage of 5,000,000 tons, which will, I assume, cause sufficient abrasion to afford a comparison between the rails tested of their respective resistances to abrasion. A speed of fifteen or twenty miles per hour by the wheels will accomplish this tonnage within three days. Testing different and differing charges of steel it would soon be known that particular methods of working the metal and treating the rails was uniformly bad, which conditions being eliminated from further tests would enable the experimenter to simplify his work, and ultimately to specify the composition of steel and the conditions of working it and of handling the product essential to the production of the most satisfactory rail.

The usefulness of such an apparatus would include experiments as to the road-bed, effects of direction of traffic, excessive loads, different kinds of wheels, etc.

Mr. H. M. Howe, Boston, Massachusetts.—We test rails to find out to what extent they possess certain mechanical properties, *e. g.* the power to resist abrasion and the power to resist sudden heavy blows, such as are given by flat wheels and by heavily laden drivers.

Intelligently devised mechanical tests give us direct and unerring information as to the extent to which such properties are possessed by the piece tested. Chemical tests give us *prima facie* evidence, indications, suggestions, as to these mechanical properties. If a piece of steel has .2 per cent. phosphorus we *believe* it to be very brittle. But if we test it mechanically we *know* whether it is very brittle or only somewhat so.

Is our knowledge of the connection between the chemical constitution and the mechanical properties of iron so complete, and are chemical tests so much more convenient than mechanical ones, as to make it desirable to reject rails on the *prima facie* evidence of their

mechanical properties afforded by their chemical composition alone, without reference to the direct evidence afforded by mechanical tests? Might it not be more logical and perhaps wiser, to merely allow the chemical composition to create a presumption for or against a set of rails, a presumption which could be refuted by sufficiently strong, direct evidence of their mechanical properties, such direct evidence, I mean, as is afforded by mechanical tests?

To take a case: Suppose a set of rails to contain only 24 phosphorus units. Suppose for the sake of argument that the manufacturers of the rails demonstrated that they had great power of resisting abrasion, great transverse strength, etc., might it not be better, in the present state of our knowledge of the subject, to take the ground that, since the chemical composition of these rails constituted a strong *prima facie* case against them, an unusually great burden of proof ought to be thrown on their sellers to prove that they are sufficiently hard, etc., by more thorough and trying tests than are required of rails whose composition does not raise a presumption against them?

I would ask whether Mr. Holley has not told us of steel made at Terrenoire, fit for rails, which carries more than .04 per cent. of silicon, which I understand to be Dr. Dudley's limit? If this be so, would it not indicate that our knowledge of the subject is still immature, since so eminent an authority as Dr. Dudley would have rejected, on account of their chemical composition, rails really unexceptionable?

During this period of immaturity should not chemical composition be admitted only as strong *prima facie* evidence, liable to be rebutted by sufficiently conclusive mechanical evidence?

MR. W. E. C. COXE, Superintendent, Philadelphia and Reading Railroad Rolling Mill, Reading, Pa.—In the remarks made by Mr. Hunt on Dr. Dudley's excellent paper, there were two points mentioned which undoubtedly have their influence in determining the life of a steel rail—that of the overheating of the steel bloom before rolling and the cold-straightening of the finished rail. Some of the rails pronounced hard and brittle may have been made so by excessive heating, and while apparently finished well, soon broke after being placed in the tracks; again, other rails while properly heated and having all the qualities necessary for a durable and strong rail, may by improper treatment in the cold-straightening have received what is known as a gag-mark, thus damaging the natural structure of the rail, placing it under strain, and rendering it unable to withstand

much wear, and liable to rupture after getting a few severe blows from engines or heavy trains passing over it. Rails damaged in this way are more susceptible to changes in temperature about the freezing-point, and would also have a tendency to fracture. The defects in these cases would be physical or mechanical rather than due to the chemical constituents of the rail, and the analysis might lead us astray in attributing the breakage to the composition of the material in the rail, instead of to the peculiar conditions created by its improper treatment. Both of these evils of poor heating before rolling and bad cold-straightening can be overcome by the exercise of care, but these accidents will sometimes occur, and the circumstances under which the rail is finished are likely to cause a condemnation of a material which might otherwise have proved itself a good article.

MR. C. E. STAFFORD, Pennsylvania Steel Works.—In confirmation of Professor Egleston's remarks, the following by M. A. Bernard, in the article, "Note on the Use of Steel Rails," in the *Annales des Mines*, Vol. V., 1876, may be of interest. He says of a table giving the number of steel rails taken up on the lines of the Northern Railway of France:

"The losses are due to broken and damaged rails. These losses go on diminishing rather than increasing at least for a long period, because, evidently, many of the breakages and damages are due to faults of manufacture. It is then from the wear, more or less rapid, of steel rails that we are to seek information in regard to their durability."

Some of the leading Bessemer steel rail manufacturers of England gave their opinion on this very subject of the wear of steel rails at the London meeting of the Iron and Steel Institute, March, 1876. Incidentally in the discussion of M. F. Gautier's paper "On the Uses of Ferromanganese," was brought forward the following:

"The question of the short life of the steel rails of the present day, as compared by that possessed by those of the early times of their manufacture, was adverted to by Mr. I. Lowthian Bell, M. P. He instanced some steel rails which were laid on the Northeastern Railway in 1862, and which stood the heaviest traffic on the road without renewal until 1874; whereas steel rails laid down six years ago had already been renewed. Mr. Snelus observed that the reason of the apparent deterioration was that there was less carbon put into rails now than there was formerly. Carbon made the steel hard, and

in consequence of that fractures had occurred. That alarmed railway engineers; they would have softer rails which, of course, were less durable . . . Mr. Menelaus observed that deterioration of steel rails was due to the requirements of engineers only. They would receive much more satisfaction in the matter of rails if they would allow railmakers to use their own judgment a little more. If they would be a little more moderate in their requirements than to insist on a rail standing one ton falling thirty feet with three feet bearings, and would, within certain limits, accept what railmakers would supply, they would get hardness combined with durability in a rail well within the limits of safety."

If the comparison of hardness or brittleness by the phosphorus-unit formula, brought forward by Dr. Dudley, be applied to rail steel out of the comparatively narrow range considered by him, it will be found to be misleading. This is owing to the fact that the various chemical elements entering into the composition of rail steel have more or less influence one upon another as their relative proportions vary, thus modifying, to a greater or less degree, their effect on the physical properties in the direction pointed out by Dr. Dudley. The effect of these different proportions is assumed to be constant in the phosphorus-unit formula; but we find in practice that the physical properties are widely different from those which we would be led to expect by its application.

One case in point: About three years ago I made an experimental phosphorus-steel heat for rails in the open-hearth furnace. The mechanical tests were the same as for rails of .35 per cent. carbon. First was taken a bar ¾ inch one way, punched out of the web of the rail, and 12 inches long. This was bent cold through an angle of 140 degrees, the ¾ inch sides being in the plane of the motion while bending.

In the second test a rail was placed head up on bearings 2 feet apart, and a ton tup was dropped on it three times from a height of seventeen feet, which bent it to an angle of about 80° from a straight line. These tests were satisfactory in every particular.

The composition of the rails was

		Phos. units.
S.	.076 per cent.	——
P.	.276	27.60
Mn.	1.245	24 90
C.	.260	8.66
Si.—not determined.		——
		61.16

This gives the hardness in phosphorus units 61.16, not taking

into account the silicon, which probably amounted to .03 or .04 per cent.

This rail stood the same tests as rails manufactured at the same time, having but 33 to 35 phosphorus units.

It may be of interest to some of the members to know more of the history of this heat. I find in looking over my notes that a little over 50 per cent. of iron rails were used, the analyses of several samples of which showed them to contain about .49 per cent. phosphorus and .091 per cent. sulphur. The steel was run into bottom-cast two-rail ingots, 10 inches square at top, 12 inches at bottom.

In hammering they had to be handled tenderly at first. After going over the surface once, the bloom was treated like ordinary steel. It was found that it hammered best at a slightly lower temperature than is usual with steel. It rolled well into rails, the heat and other conditions remaining the same as usual. Rails similar to the above have been laid down on English and French railroads, and are giving good results.

Mr. William Kent, Pittsburgh.—Wearing power is not dependent upon hardness alone, but upon both hardness and cohesion. Thus an alloy of copper and tin (speculum metal), is so hard that it cannot be scratched with a file, yet in attempting to grind it on a grindstone it instantly flies in splinters. It is intensely hard, but throughout its structure there exist planes of weakness through which the material may be broken with almost a touch. A mass or structure can be imagined consisting of very small diamonds, held together by some weak cementing material. The diamonds are hard enough, but the structure which they make might be so weak that it would not resist abrasion. It may be the same with hard steel rails. The steel may contain enough carbon, phosphorus, and other "hardeners" to be very hard, and even to have great tensile strength, but there may exist throughout the structure such planes of weakness between the hard particles, that these particles might be easily abraded. Hardness may or may not be accompanied by brittleness and defective wearing power.

So much has been said in reference to the influence of chemical composition on wearing power that Dr. Dudley's physical tests, made with the Thurston torsion machine, seem to have been lost sight of. Of the rails which crushed or broke in service, in every case but two, the tensile strength was 75,000 pounds or above, and the elongation 20 per cent. or below, while of the rails which did

not crush or break in service, with but one exception, the tensile strength is between 65,000 and 75,000 pounds per square inch and the elongation 21 per cent. or above. If, as Dr. Dudley says, the specifications had required that rails should have a tensile strength of over 65,000 pounds and an elongation of over 20 per cent., every rail without exception which crushed or broke in service would have been rejected. Referring to the diagram it will be seen that every bad rail had a low percentage of elongation, 20 per cent. or below, while every good rail had a high percentage of elongation, 21 per cent. or above. The physical tests, therefore, give a very plainly marked dividing line between the good and bad rails. If upon more extensive experiments it should be found that this relation of the results of physical tests to wearing power is general and not accidental, why not base the specifications upon physical tests alone, and not upon chemical analysis? If, for instance, it could be proved that a rail of which a test specimen would show between 65,000 and 75,000 pounds per square inch tensile strength, and over 20 per cent. elongation, was always a good rail, and that a rail of which the test specimen had either under 65,000 or over 75,000 pounds tensile strength, or under 20 per cent. elongation, was always a bad rail, what would it matter to the railroad companies if the first rail contained .2 and the second only .1 phosphorus? If a constant relation can be found between the wearing power and the results of physical test, and if the physical test can be cheaply and conveniently made, it would appear sufficient for the railroad companies to make the physical test the basis of the specification, and leave to the steel manufacturers the more difficult question, from both the scientific and commercial standpoints, of determining what should be the chemical composition of the rail.

Mr. A. S. McCreath, Chemist of Second Geological Survey of Pennsylvania, said (referring to the discussion of the effect of other elements than those referred to in Dr. Dudley's papers), I have analyzed some steel made from Cornwall pig iron, which contained over half of one per cent. of copper. The steel worked well, there was no trouble in the rolling mill, and the rails were sent out of the works as standard No. 1 rails. I have also analyzed Bessemer steels containing quite a large percentage of chromium—an élement which seems to have been overlooked in the discussion. The chromium made the steel very hard, and when present in an appreciable quantity, the percentage of carbon had to be reduced so as to prevent the

steel from becoming too hard and unfit for rails. I have found arsenic in some irons, but the amount present was very small and its effects were not observed.

Dr. R. W. Raymond, New York.—It has sometimes seemed to me that the supposed difference between hardness and cohesion is simply one of the size of the units of structure and their homogeneity or symmetrical balance of internal tensions. A substance like glass, for instance, may be pronounced hard when its molecules resist separation by the projecting points or edges of a file, so that the one being pressed against the other, the molecules of the file give way first. The units of structure here acted upon in the glass are small, and the effect of vibration in developing lines of weakness due to internal tensions is not perceptible. But striking the glass with the same file we find it is brittle. It flies to pieces, because the lines or surfaces of weakness separating its larger units are developed by vibration, and because, moreover, the force actually brought to bear is much greater, and cannot be, in this case, relieved by the separation of the molecules on the file. They are merely compressed by the blow. Now the multiplying effect of vibration, concentrating the molecular motions at single points, is well known; and it is not to be wondered at, if bodies which are both hard and highly elastic, even though hardness be but a manifestation of cohesive power, should show what we call brittleness. Finally, the wear of a rail is not merely abrasion, but hammering also. If cars had runners instead of wheels, those rails which were hardest would undoubtedly wear longest (apart from breakages). Hence the test of the quality of surface required for rails should include something more than mere abrasion; and in testing by abrasion to determine hardness, due regard should be had to the size of the units acted upon; and this depends upon the condition of the surfaces brought together, and the nature, direction, and speed of the pressure and motion imparted.

It is self-evident that Dr. Dudley's paper covers, and was meant to cover, but a limited range. As he is well aware, thousands of tons of serviceable steel rails have been manufactured lower in carbon and higher in phosphorus than the most extreme cases he has included. But these have been made in the open hearth; and so far as the ordinary Bessemer manufacture is concerned, Dr. Dudley has covered the range of that process as it is represented in the rails of the Pennsylvania Railroad. We all agree that in the determination of the limited data available for his purpose he has been thor-

ough and precise; and we must admit that he has a right to try to express the results in a formula which, within certain limits, will give the chemical constitution of a good rail. To meet this formula by adducing instances, within the same limits, which it does not explain, is legitimate criticism; and if the data are equally certain and complete, comprising not only analyses but physical tests and the tests of actual service, the criticism, I think, would force either the change of the formula or the change of the limits for each ingredient within which it is supposed to be practically correct. But to say that it is a mere guess, and that any other guess might do as well, is to ignore the mathematical argument which attaches a high degree of probability to a series of coincidences. Whether a phosphorus unit, or a "hardener" unit, or a steelifying unit of measurement be possible for the practical guidance of the manufacturer and purchaser is indeed an open question. But experience shows that there is a relation of some kind between the ingredients of a good rail; that to maintain the desired quality one ingredient must be increased or diminished as another is varied.

The expression of this relation may be far more complicated than Dr. Dudley has suggested; but if it is to be found at all, it must be found by guessing, which is a recognized step in the processes of the inductive philosophy. A hypothesis is a guess, and Dr. Dudley's guess harmonizes with his facts, which is no small thing. Here we have twenty-five equations, each containing five unknown quantities, and Dr. Dudley's values for these quantities satisfy all the equations. I have no doubt he guessed a good deal and tried a good many solutions before he came down to this one. I notice, for instance, that the numerical relation between carbon and phosphorus, put forward, I believe, at Terrenoire, is not adopted by him; and I presume it was discarded because it would not fit his data. Gentlemen who underrate the labor involved in arriving at such a hypothesis, and deem it easy to guess out a formula like this, are invited to give us, out of their abundant facility, a guess that will do half as well.

These remarks are not an indorsement of Dr. Dudley's formula, but rather a protest that it should neither be overrated as to its claims nor underrated as to its merits.

MR. WILLIAM P. SHINN, Edgar Thomson Steel Works, Pittsburgh, Pa.—In my opinion Dr. Dudley has been very seriously misunderstood or misrepresented in regard to the views expressed by him in his two papers read at the Lake George meeting, regarding

the chemical and wearing qualities of steel rails. It has been generally supposed that Dr. Dudley claimed to have proved the correctness of a theory, and to have established a principle in regard to the comparative hardening value of the chemical elements, phosphorus, silicon, carbon, and manganese, giving them relative values in the order of their names of 5, $2\frac{1}{2}$, $1\frac{2}{3}$, and 1 respectively; or 1, $\frac{1}{2}$, $\frac{1}{3}$, and $\frac{1}{5}$, the "1" in the latter representing a so called phosphorus unit, and claiming also that the hardening effect due to their aggregate presence in steel would be found to be equal to the sum of their several separate effects and the hardening effect of the corresponding percentage of phosphorus as represented by the scale above referred to.

So widely has this idea become disseminated that I have had within the past month letters from three parties with whom we had contracts requesting that their rails be made "according to the Pennsylvania Railroad specifications," as described in a certain paper (referring to a republication of Dr. Dudley's paper), and stating that rails constituted as provided in Dr. Dudley's formula "had been found to give the best results in practice." I do not understand that Dr. Dudley has assumed to have established a principle, but that on the contrary, he has simply brought forward an hypothesis, the correctness of which remains to be established, and in so doing, I think that Dr. Dudley has rendered very valuable service, both to the manufacturers and to the consumers of steel rails. While it appears from the paper referred to that the hypothesis therein stated does reasonably well account for and correspond to the difference in physical characteristics exhibited by the rails under investigation, yet it must be stated as a very plain criticism thereon, that the number of specimens investigated was entirely too small to prove a theory, and that the attempt to fix the chemical qualities of 500,000 tons or more of steel rails, which are to be manufactured this year, upon an investigation covering twenty-five specimens, is building a pyramid upon its apex. Not only do I consider it very doubtful whether the hardening effects of the several designated elements are in the ratio of the numbers stated, but still more do I doubt whether we have sufficient evidence upon the subject to even warrant an hypothesis that the combined effect of the several hardeners is equal to the sum of their several effects when taken individually. On the contrary, it is within the experience of most steelmakers that as the phosphorus goes up the manganese must also be increased, and (as is well known to be the claim of the Terrenoire process of manufacturing high phosphorus steel) that if the carbon be kept down at the same time no

undue hardening takes place. To show that the physical characteristics as to tensile strength and elongation do not always correspond to the phosphorus units, as claimed in general terms by Dr. Dudley, I present herewith a statement of ten specimens of steel rails which were procured for Dr. Dudley, and tested by him, or under his direction, on the Thurston torsion machine, with the results given in the table below. I have had them carefully analyzed by three chemists, and present opposite to each the number of phosphorus units according to Dr. Dudley's formula, as ascertained by each of the three chemists, as follows:

Order of elongation.	No. of specimen.	Height of diagram. Inches.	Length of diagram. Inches.	Elongation. Per cent.	Phosphorus Units Determined by				Order of phosphorus units.	Classed by torsion test.
					P.	F.	W.	Average P. and F		
1	27	3.79	15 66	31.44	46 97	50 41	51.09	48 69	9	Good.
2	18	3.78	14 97	29.00	40 77	49 17	52.87	44 97	3	"
3	24	3 78	14.77	28 40	42 43	46.25	....	44 34	2	"
4	21	3 80	14 56	27.69	49.50	50 17	....	49.83	10	"
5	20	3 80	14 20	26.48	47.04	50 13	....	48.58	8	"
6	17	3 80	13 75	24.99	48.02	49.02	....	48.52	7	"
7	26	3.39	7.72	8.46	42.76	45.91	....	44.33	1	Bad
8	15	2.98	3.00	1.32	44 12	49.21	54.08	46.66	4	"
9	25	2 90	2.40	0.85	50.49	45 30	50.88	47.89	5	"
10	23	2.74	2.12	0.65	49.36	46.46	53.79	47.91	6	"

The above table gives the length (corresponding with the elongation) and the height (corresponding with the tensile strength of the specimens) in the order of the greatest length or elongation.

While the determinations of the three chemists referred to (all of whom are in good repute as chemists of Bessemer works) do not agree with that exactness that is certainly desirable, yet they agree substantially in indicating that the phosphorus units do not at all correspond to the elongation of the specimens. As will be noticed, the first two specimens, Nos. 27 and 18, give diagrams respectively, 15.66 and 14.97 inches in length, while the last two specimens, Nos. 25 and 23, give diagrams respectively, 2.40 and 2.12 inches in length; and yet the phosphorus units of No. 25 are shown by all the chemists to be less than those of No. 27, while

the phosphorus units of No. 23 are shown to be a little in excess of Nos. 27 and 18 by two of the chemists, and even less by the third. The appearance of the specimens themselves certainly indicates a very great difference in physical characteristics, while the difference in the various chemical elements between Nos. 27 and 18 are quite as great as between Nos. 18 and 25, the two former numbers, 27 and 18, being the highest two of the series, while No. 25 is next to the lowest in elongation. It would be interesting to pursue this subject further, but I have not the data to enable me to form any satisfactory conclusion, but the table above given shows that Dr. Dudley's hypothesis is far from being applicable to the specimens therein referred to.

Again I notice in Dr. Dudley's reference to the paper of R. Price Williams, he gives the following as indicating the substantial correctness of the phosphorus unit theory, one rail of each pair being designated as hard and the other as soft:

Hard.		Tonnage per 1-16 inch wear.	Soft.		Tonnage per 1-16 inch wear.
No.	P. U.		No.	P. U.	
17	38	5.251.000	18	30	8.402.000
23	47	15.531 000	24	25	31.061.000
22	40	9.283 000	21	33	7.676.000

In the above series it will be seen that while of the rails designated as soft, two show a higher tonnage duty than those designated as hard, the third, No. 21, shows a lower tonnage duty than No. 22 designated as hard, while if No. 23 be compared with No. 18 it will be seen that there the hard rail has given nearly double the tonnage duty of No. 18 designated as a soft rail. While there may not be, and indeed I do not claim that there is, anything in this to prove that the hard rail wears better than the soft, I refer to it simply as indicating that it is far from proving that soft rails invariably, or even generally, wear better than hard.

As a manufacturer of steel rails I am deeply interested in all that tends to throw light upon the complex questions involved in their chemical constitution and physical manipulation, and shall welcome all investigations of the character of that made by Dr. Dudley; but I feel that it is quite important that we should not hastily jump to a conclusion which may leave us in a worse position than before the investigation. As it is we should investigate with the view of endeavoring to arrive at a proper and satisfactory conclusion.

As a matter of interest to the members, I will here mention that the fall of the Glasgow Bridge, in process of erection over the Missouri River, has indicated remarkable capacity for resistance to shock

on the part of the steel therein employed, which was of the variety known as "Hay" steel, manufactured by the Edgar Thomson Steel Company, Limited, by the Bessemer process. It was at first announced that no members had been broken by the fall, but the facts as subsequently ascertained, while not quite so remarkable as this would indicate, are sufficiently so to admit of repetition here.

A span of that bridge, 314 feet in length, was so nearly completed that in six hours more it would have been self-sustaining, when the false works were taken out by ice and the whole structure fell into the Missouri River by buckling in the centre. The height of the top chord was 102 feet and of the bottom chord 72 feet above the water. The total weight of steel on the false works was about 160 tons. Of that amount there was

Entirely uninjured, about,	50 tons
Requiring slight repairs,	30 tons
Requiring heavy repairs,	20 tons
Requiring to be renewed,	60 tons

Of which latter 20 tons consisted of eye and chord bars which were only bent. Of 213 eye-bars (rolled upon Kloman's mill), only four were broken. Careful investigation showed that there was not a single break which started from a flaw, and not a rivet broken in the whole structure, the rivets being of the same material as the rest of the structure.

This is certainly a very satisfactory test of the first all-steel bridge in this country.

Dr. Chas. B. Dudley—In replying to the criticism which has thus far been made on my work on steel rails, I wish first of all to express my high appreciation of the very kind and considerate tone which has pervaded everything that has been uttered, and my hope that in what I may say I shall be equally fortunate in manner.

I have made a few notes during the progress of the discussion and will speak from them. And in the first place I should like to say that I think broken rails are not the important matter they were a few years ago. When I tell you that the total number of broken rails on the Pennsylvania Railroad Division of the Pennsylvania Railroad, including about 1650 miles of track, was, during the year 1878, 43 rails, and that four years ago the number for the same division was 273 rails, I think you will agree with me that in this day of railroads broken rails are a small matter, and that the question for the rail manufacturers is not now simply to make rails

which will not break in track. The reason for this state of things is that during the last five years that department of railroad management which is called Maintenance of Way, as well as almost every other department, has vastly improved in efficiency. I am sure I speak what every Pennsylvania Railroad man knows to be true when I say that the present standard of maintenance of track is 50 per cent. in advance of what it was five years ago. The truth is that a well-spiked rail, laid on good white oak cross-ties, eighteen inches from centre to centre, with stone ballast well rammed under the ties, and double angle splice-bars with nuts well screwed up, is a tremendously strong combination, and one capable of resisting enormous strain. I have never heard any estimates of the factor of safety in such a track as this, but it must be very high.

We have recently had made at Altoona a series of physical tests of rails taken from track, and out of a series of some fifty or sixty tests representing as many rails, no less than six proved on test to be little better than cast iron. The fracture was almost exactly like that of fine, close cast iron; the tensile strength, it is true, was higher, but the elongation was only a little greater than good cast iron ought to give. And yet these rails were in the track for over two years, and that, too, without causing accident. I think it safe to repeat, therefore, a well-spiked rail on good cross-ties, with track in good order, is a combination of very great powers of resistance, and that the matter of broken rails is not the main question.

On the other hand *crushed rails* are a very much more important matter. By crushed rails I mean rails which split or laminate, or go to pieces in the track. And here again I am sure I speak what every man connected with Maintenance of Way of Pennsylvania Railroad knows to be true, when I say that more rails are taken from the track on account of being crushed than from any other single cause.

A new rail is laid in track and subjected to service. Now with all the care we are able to bestow it is not always possible to prevent low joints, and the consequence is that every time a wheel passes over this joint the end of the rail gets a blow. Or, again, a wheel becomes flat from sliding, and every time this flat place comes around there is a blow on the track. In addition to these blows there is constantly the great weight of the locomotives and cars tending to crush and disintegrate the steel in the rails, and the sidewise thrust of the mass of the locomotives or the cars, especially at curves and at high speed, which in combination with the weight and possibly with

the blows before mentioned, puts a strain on the head of the rail which it needs good metal to withstand. What do we find to be the effect of these influences on the new rail? Sometimes after only six or eight months, and sometimes after a year and a half, and sometimes after a longer period, the new rail either splits at the end, or pieces split off from the sides of the head, or at different places along its whole length it becomes battered down, laminated, and crushed. Some rails never batter or crush, never show any signs of disintegration, their whole life is made up of honest wear, while others go to pieces in the manner described within a year from the time they are first laid in the track.

Now as to the cause of this crushing and going to pieces of the rails there seems to be a difference of opinion. The steel-rail manufacturers seem inclined to think it is almost entirely due to physical defects in the steel. Some say that the "bottom-cast" is the cause of it, and some that whatever method of casting you use it is not always possible to secure solid ingots, and that pipey ingots, or ingots full of blow-holes, will give rails in which these pipes or blow-holes are simply lengthened out, and consequently there is a physical defect in the rails which requires only the strain of service to develop into what may be called a crushed rail. Indeed, one of the presidents of one of the important steel works on the line of the Pennsylvania Railroad, recently said to me: "The pictures of the crushed rails which you show in your report are nothing else than what we call 'split ends,' and are due to imperfections in the ingot. If these rails had been cut off two or three feet shorter they would never have crushed."

In reply to these criticisms I beg leave to say, that while I do not deny that physical defects may, and undoubtedly in many cases do, occasion failure in the rail, I am far from being ready to allow that another and deeper cause is not responsible for a large amount of this failure. As I said in reply to this same president, "In my opinion what you technically call 'split ends,' and say is due to imperfections in the ingot, is nothing more than a scapegoat which has been made to bear the sin of an enormous amount of poor steel." And my reasons for thinking that imperfect ingots are not the sole cause of crushed rails are these: If any of you will take the physical tests of the series of rails described in the report which we are discussing, and subject them to a little examination, you will find, I think, a clearly marked difference in tensile strength and elongation between those rails which did not crush or break in

service and those which did break or crush in service. Again, if you will turn over to the analyses of the series you will find there just as clearly marked a difference between the crushed and uncrushed rails in chemical composition. I submit to you therefore the question. If we find in a series of steel rails, part of which have given good service and a part bad service, a clearly defined difference in the chemical composition and physical properties of the steel, is it not fair to attribute at least a part of the failure to the quality of the steel itself? Every one knows that a hard, brittle metal is more liable to crush and disintegrate under blows and strain than a softer, tougher metal. So, when you say to me that if a rail had been cut off two or three feet shorter it would never have crushed, I reply, if you will come with me and walk over the tracks of the Pennsylvania Railroad, as did my friend Captain Jones of the Edgar Thomson Steel Works a few days ago, I will show you rails which, according to this dictum, would need to be cut in two in the middle, and both ends thrown away; I will show you rails sound at both ends and crushed in the middle; and if this fails to satisfy you that physical defects are not the only cause of crushed rails, I will show you rails more or less crushed or disintegrated throughout their whole length. I repeat, therefore, that while I am not willing to say that I think physical defects are not responsible for a part of the trouble, I am willing to say that I think the quality of the steel must be held accountable for no small share of the failures. And just here I may be permitted to say that, in my judgment, the ultimate criterion by which a steel rail should be judged, the one by which consumers are already beginning to judge rails, and by which they will judge them more and more in the future, is not by what the rail does not do, but by what it does do; not by the fact that the rail does not crush or break in service, but that it endures long and hard service. To railroad men the important question is: After the track is once equipped with steel rails, how long will it be before those rails must be renewed? And in these times when railroads, like all other lines of business, are looking most vigorously after their expenditures, the question of renewal of rail equipment comes home with a force that it is impossible to pass by unheeded. And it seems to me—let me say it with all modesty—that the direction in which our efforts to secure rails of longer life must tend, is at least indicated by the conclusions as to chemical composition which are embodied in the report which we are discussing.

A moment now as to Mr. Hunt's criticism of my statement that "the less silicon in the rail the better." I see nothing in the data which Mr. Hunt brings forward to cause me to seriously modify or change that statement. I am entirely ready to confess that there is nothing in my results to show that a rail with more silicon in it would not give good service. And still it seems to me that my statement is defensible notwithstanding. That statement was made with this thought in mind, that of the four substances affecting the quality of steel—carbon, phosphorus, silicon, and manganese—the silicon is the one which, with our present metallurgical methods is easiest reduced to a minimum. If now I am correct in believing that steel rails made in this country according to present Bessemer practice contain already too much hardening material, I think no one will say that, as long as the other hardening constituents remain as they are, the silicon should be increased. In other words, of the impurities existing in pig iron, which the conversion into steel simply diminishes in amount, the phosphorus we cannot remove, a certain amount of carbon and manganese are considered essential to the successful manufacture of the steel, and these three hardening constituents being according to present practice, in my opinion, already too high, it seems to me nothing but common sense to say in reference to the silicon, which with present methods we are able to reduce to a minimum, the less in the rail the better. As to the question whether it would not be advisable to diminish the amount of carbon and manganese a little and increase the silicon, I am unable to express an opinion; but I hardly think even Mr. Hunt would have us think so for no stronger reason than that he happened to find from one-tenth, to three-tenths of silicon in a number of samples of crucible steel, in which, if I am right, the silicon is there not because it is wanted, but because the crucible steel makers cannot help it; nor because one rail with a full amount of other hardening constituents has a little over four-tenths of silicon, and has not broken or crushed in service.

As to the question of phosphorus units, there seems to be a little misunderstanding of my position. I most devoutly believe that if the different substances affecting the quality of steel, act alike in any one particular, it is not only correct but philosophic to take into account their combined influence when we attempt to measure the value of the steel in that particular. In other words, if carbon, phosphorus, silicon, and manganese each and all tend to make a piece of steel brittle, it is entirely correct, in judging of the value

of a piece of steel as to its brittleness, to take into account the combined influence of these substances. Still further, if equal amounts of each of these substances make steel brittle in different degrees, it is obvious that to get a correct measure of their combined influence we must estimate them all in the same unit.

Now, that each of the substances, phosphorus, carbon, silicon, and manganese, do tend to make steel brittle, I think there is very little doubt. The everyday experience of every Bessemer works in the country affords evidence of the fact that if these substances are in excess in a piece of steel, that piece of steel will be cold-short or brittle. That I have hit upon the right numbers by means of which to estimate the combined influence of these substances on the steel, I have never said and do not claim. The hypothesis of phosphorus units is offered as a means of measuring steel which, when the right numbers—be they those which I have suggested or not— are ascertained, will prove, as I think, of no small value. But again, I hope no one has understood me as saying, or meaning to convey the idea, that I think two pieces of steel, identical in phosphorus units, will have physical qualities identical in all respects. For example, a piece of steel in which the phosphorus units were principally made up of phosphorus, would undoubtedly have a different tensile strength from one in which the same number of phosphorus units were principally carbon; or, again, a piece of steel in which the phosphorus units were principally phosphorus would undoubtedly work differently and weld differently from a piece in which the same number of phosphorus units were principally silicon. Or, once more, a blow of steel in the ladle in which the phosphorus units were principally manganese would undoubtedly cast better than one in which the same number of phosphorus units were principally phosphorus. The examples might be largely multiplied, but perhaps enough has been said to make the point clear. What then, you say, do you mean by phosphorus units? I reply that the conception to which I have tried to apply the hypothesis of phosphorus units, is that peculiarity of these substances by which they make steel brittle. And when I say a piece of steel is brittle, I use the word in its primary signification, viz., that a brittle piece of steel will not suffer much distortion without rupture. The ultimate question, therefore, to which the conception of phosphorus units leads is this: Will a piece of iron containing three-tenths of a per cent. of phosphorus and nothing else; a piece of iron containing six-tenths of silicon and nothing else; a piece of iron containing nine-tenths of carbon and noth-

ing else; and a piece of iron containing one and a half per cent. of manganese and nothing else, I say, will these four samples of iron or steel, whichever you choose to call them, all give the same elongation? Of course I have no experimental data to show that such would be the case, and, as I said before, I simply offer the conception of phosphorus units for your consideration and criticism. It must stand or fall on its merits, aud there I am content to leave it.

A few words now in reference to the investigation which has led to this discussion, and I am done. There have been few investigations made, I think, in which there was less bias from previously formed opinions or prejudices than this, and the investigation was undertaken with not more than half a hope on my part that it would lead to anything valuable. Indeed, I plainly told the officers of the road that I might work six months at the problem, and spend a thousand dollars on it, and then not be able to tell them a single thing that would be of use to them. Still further, I did not myself select the rails of the series; they were sent me by number, and I knew almost nothing about them until my work was finished. The question proposed to me was: Is there a chemical and physical difference between rails which have given good service and those which have not given good service? The physical tests were of course made first, and then borings for analysis were taken from the test-pieces. The chemical analyses were then made, and finally the tonnages and complete history of the samples were computed and written out. The results of the physical tests were not computed, nor was any attempt made to study the results at all until the work was all done. Mr. Ely, Superintendent of Motive Power, asked me, when the analyses were finished, what I knew, and I told him I knew nothing. Gradually, however, as the results began to be studied, as the results of the physical tests began to be compared with the analyses, and as the tonnages began to come in, the whole matter began to take shape. Everything fell into line, and it almost might fairly be said that the results of all the work arranged themselves in the shape in which they are presented in the report.

So much for the work and the manner of doing it. But you here say, and the same in effect has been said to me by others, "We do not quarrel with your work, nor with the results you have obtained, but with the conclusions which you have drawn from them." In reply I say there are twenty-five analyses of steel rails which have been in actual service, made with all the care and skill which I was capable of using; there are twenty-three physical tests which were made by

Mr. J. W. Cloud, who has charge of the department of Physical Tests at Altoona; and there are the tonnages and history of each rail of the series which have been prepared as carefully and accurately as it was possible to obtain them. And now if you do not like my conclusions, draw your own conclusions. And how any man who is candid and fair can draw any other conclusions from the data than these, viz. (1.), that there is a physical difference, and there is a chemical difference between rails which have actually given good service and those which have given bad service on the Pennsylvania Railroad, and (2) that this difference consists in the fact that the good rails contain less of the hardening constituents, carbon, phosphorus, silicon, and manganese, than the bad ones, is more than I can comprehend. Indeed I do not see how it is possible to escape these two conclusions, however you look at the results.

If you do not like my division of the rails into those which crushed or broke in service, and those which did not crush or break in service, divide them strictly according to the tonnage. Call the eleven, or twelve, or thirteen of the rails with the highest tonnage good rails, and the remainder poor rails, and then take the average results of the analyses and physical tests of each series, and still you have the same conclusions. Still further, throw the question of phosphorus units entirely out of the report and make your formula to guide you in your practice by taking the average of the analyses of the good rails, dividing the series into good and bad rails, as I have done, or according to the tonnages, as above described, just as you choose, and still you have the same conclusions, viz., that the good rails differ from the bad rails in chemical composition; and this difference is that the good rails have lower carbon, phosphorus, silicon, and manganese than the bad ones.

These two conclusions seem to me to embody the most important results of the work which we are discussing, and I hardly see how it is possible to escape from them. The only unanswerable criticism that has been made to-day is, that the series of rails analyzed is not large enough to warrant us in drawing satisfactory conclusions from them. I am free to confess that I wish as heartily as you can that the series had been larger; but I think the series is large enough to warrant us in regarding the conclusions which it teaches as indications, and pretty satisfactory ones, too, of the direction in which our efforts must tend in trying to secure better rails.

My friend, Captain Jones, said a short time ago: "We know that the formula suggested in the report will give us a good rail, and our

efforts as steelmakers should be to see if some other formula will not give as good a rail." To which I say, yes, with all my heart; but submit to you, if in the mean time, while the experiments at securing good rails on other formulæ are being made, it is not only good sense, but also wisdom and good policy for the Pennsylvania Railroad to ask to have its rails made on a formula which its experience teaches it will give a satisfactory life and wear.

Mr. J. W. Cloud, of Altoona: For the purpose of drawing the most valid conclusions that can be obtained from so limited a number of specimens of Bessemer steel as those included in Dr. Dudley's interesting and very instructive paper, read before this body in October of last year, I have taken 23 of his 25 analyses, this being the total number of samples of which torsion tests were made, and have formed 23 equations between the content as shown by chemical analysis on the one side and the length of diagram given on the torsional machine on the other side, and have solved the 23 equations thus formed by the method of least squares for the most probable value of the influence of a unit of each substance in the steel on the length of the diagram.

A second series of equations similarly formed, but with height of the diagram substituted in the right hand member of the equations in place of the length of diagram, has been solved in the same way for the most probable value of the influence of a unit of each substance found in the steel on the height of diagram.

Again, a third series of equations with height of diagram at elastic limit for the right hand member of each equation has given me the value that can most rationally be assigned, from the information before us, to the influence of one unit of each substance in the steel on height of diagram at the elastic limit.

Before stating this more in detail, let me recall to you the following facts about the diagram given on the torsional machine. The diagram obtained is an automatic record showing the twisting moment that the test-piece was subjected to at every instance during the test, together with the angle through which each torsional moment has twisted one end of the piece relatively to the other end. It therefore gives from the height of diagram, at the breaking-point, the torsional moment required to break the piece, and from the total length of diagram it gives the total angle through which the piece had to be twisted to break it with these torsional moments. The former is approximately proportional to the tensile strength of the

metal, while the latter gives directly the maximum elongation of an outside fibre which is greater than the elongation in a tensile test, but approximates to the maximum elongation in tensile test, which occurs in a differential portion of the original length at the point where fracture finally takes place, but which we never measure. We distribute this over some portion as 2, 4, or 5 inches on the whole length of the bar, and therefore get a reduced percentage of elongation in tensile test. The diagram also gives information about the strength of the metal at elastic limit, together with other peculiarities at this point which could only be determined by the finest and most elaborate measurements from a tensile machine. In short, the torsional diagram, when thoroughly understood, is a more complete indication of the qualities of the metal than any other single test that I am familiar with to which it can be subjected.

To resume after this digression, I have assumed entire absence of all impurities not determined and not reported in the analyses, and have taken the iron in each by difference between the sum of carbon, phosphorus, manganese, and silicon, and one hundred per cent. Consider now one one-hundredth of one per cent. of each of the five substances in the steel as a unit of that substance, viz.: Fe., C., P., Mn., and Si. respectively—also consider $\frac{1}{100}$th of 1 inch a unit of measurement on the diagram, and let

u =	influence	of a	unit of	Fe.	on the	length	of diagram.
v =	"	"	"	C.	"	"	"
x =	"	"	"	P.	"	"	"
y =	"	"	"	Mn.	"	"	"
z =	"	"	"	Si.	"	"	"

Supposing the process of manufacture to be constant in all the minute details of manipulation which influence the physical properties of the steel, and that those properties are modified solely by the variation in two or more of the constituents, we can write the following equations, if it is understood that the results of their solution are provisional on the foregoing assumptions, and that they only apply to steels in the range of variation of ingredients found in the analysis, for the influence of the impurities *per se* may vary outside of this according to other laws of which we can take no cognizance.

	Length.	Height.	Elastic limit.
9906 u + 34 v + 8 x + 46 y + 6 z	= 1370	= 311	= 115
9924 u + 28 v + 11 x + 34 y + 3 z	= 1270	= 279	= 107
9923 u + 29 v + 6 x + 35 y + 7 z	= 1260	= 292	= 118
9945 u + 23 v + 4 x + 21 y + 7 z	= 1750	= 270	= 108
9927 u + 31 v + 6 x + 33 y + 3 z	= 1480	= 282	= 103
9912 u + 35 v + 8 x + 41 y + 4 z	= 1240	= 302	= 111
9927 u + 23 v + 9 x + 36 y + 5 z	= 1510	= 284	= 118
9933 u + 22 v + 11 x + 32 y + 2 z	= 1300	= 272	= 103
9919 u + 29 v + 8 x + 42 y + 2 z	= 1490	= 312	= 160
9891 u + 35 v + 10 x + 58 y + 6 z	= 1340	= 328	= 122
9941 u + 22 v + 7 x + 27 y + 3 z	= 2170	= 275	= 107
9918 u + 30 v + 17 x + 32 y + 3 z	= 1200	= 312	= 125
9882 u + 34 v + 13 x + 67 y + 4 z	= 1210	= 310	= 118
9916 u + 29 v + 18 x + 35 y + 2 z	= 1170	= 323	= 120
9902 u + 37 v + 13 x + 46 y + 2 z	= 820	= 250	= 94
9832 u + 57 v + 8 x + 85 y + 18 z	= 1010	= 423	= 169
9883 u + 35 v + 13 x + 63 y + 6 z	= 1050	= 332	= 130
9898 u + 32 v + 14 x + 52 y + 4 z	= 1060	= 330	= 131
9905 u + 39 v + 13 x + 38 y + 5 z	= 850	= 332	= 120
9893 u + 42 v + 16 x + 46 y + 3 z	= 1020	= 336	= 116
9913 u + 30 v + 14 x + 41 y + 2 z	= 1020	= 271	= 95
9885 u + 39 v + 6 x + 67 y + 3 z	= 670	= 296	= 114
9894 u + 36 v + 16 x + 51 y + 3 z	= 1110	= 323	= 120

The coefficients of the several unknowns, u, v, x, y and z, on the left side of the equations being the number of units of each of these substances in the steel, and the three values in the right hand member being the length, height and elastic limit of the diagrams respectively, we have three separate series of equations to be solved independently, as already explained, by the method of least squares. They are put down in this way simply to economize space.

The solution of these equations gives the three following systems of values with their proper and necessary signs for u, v, x, y and z.

	Elongation.	Tens. strength.	Elast. limit.
u	+ .237	+ .019	+ .008
v	— 29.54	+ 1.191	+ 1.191
x	— 15.8	+ 3.212	— .409
y	— 3.694	+ .342	— .086
z	+ 39.8	+ 6.292	+ .548

These values substituted back into the original equations, and the figures obtained compared with the original right member, show the following:

LENGTH.		HEIGHT.		HEIGHT AT ELASTIC LIMIT.	
Measured.	Calculated.	Measured.	Calculated.	Measured.	Calculated.
1370	1287	311	309	115	116
1270	1346	279	288	107	107
1260	1551	292	298	118	112
1750	1816	270	280	108	107
1480	1341	282	275	103	113
1240	1198	302	295	111	116
1510	1598	284	289	118	103
1300	1493	272	274	103	99
1490	1294	312	276	160	108
1340	1178	328	319	122	115
2170	1616	275	266	107	102
1200	1198	312	308	125	107
1210	1046	310	328	118	111
1170	1161	323	305	120	105
820	960	250	302	94	115
1010	925	423	422	169	146
1050	1111	332	330	130	113
1060	1148	330	314	131	109
850	1050	332	321	120	120
1020	802	336	324	116	120
1020	1171	271	296	95	107
670	970	296	295	114	119
1110	961	323	318	120	113

These values and signs for u, v, x, y and z, mean that the substance represented by each of these letters increases or decreases the measurement of the diagram under consideration according as the sign is plus or minus, and at the rate given by the figures. I wish to be understood, however, in saying that these values are only tentative—that they are the most probable values from the data before us, and that they should be corrected by the application of the same method to a larger number of samples where special care has been taken to make the manufacture constant and to get good solid uniform test-pieces. I do not anticipate, however, that these values will be very greatly altered quantitatively, and perhaps very few if any of them changed qualitatively, *i. e.*, in sign.

If by this method I have succeeded in disentangling the bundle of influences in each of the twenty-three equations, and in ascribing to each element its most probable share in quality and magnitude in the final result, I have succeeded in what I have undertaken, and the above table of values becomes useful until it can be made more accurate by a greater number of samples, examined with a special view of establishing correct values, which would necessitate an accurate determination of all the impurities in the steel.

It would appear from these values that manganese is a miniature phosphorus, so to speak, *i. e.*, it has *similar* influences on the elastic limit, tensile strength and elongation, as shown by the same signs in each case, but the influence is of much less magnitude than phosphorus, its effect in decreasing elastic limit and elongation being one-fifth as great as that of phosphorus, while its effect in increasing tensile strength is only one-tenth as great as that of phosphorus. Further, it appears that manganese is the only one of those in question which has similar influences to phosphorus all through; carbon is similar to phosphorus as to tensile strength and elongation, but it has a contrary sign to phosphorus, and a value of large magnitude as regards elastic limit. Further, silicon is opposed to phosphorus, and an antidote to it and manganese as regards elongation and elastic limit, but is with them in increasing tensile strength, and that at a high rate.

Unfortunately I have not yet a large number of samples, not included in above equations of analyses and physical tests, to apply these values to for verification or otherwise, but by the date of the next meeting of the Institute I hope to be able to communicate something more in this direction.

DR. DUDLEY, said he would make a few adverse criticisms as a means of calling more attention to this matter. He did not quite agree with Mr. Cloud's mathematical views. He questioned whether there is anything equatable between the chemical contents of the steel expressed in hundredths of one per cent. and the elongation of the diagram expressed in hundredths of inches. To his mind a diagram, being a record of the tensile strength and elongation, is a resultant belonging principally to the iron modified by the constituents in the steel, namely, carbon, phosphorus, silicon, and manganese. If a diagram obtained from pure iron could be checked with the test diagrams published, and the differences between them could be reached, figures equatable with the constituents entering

into the steel could be got. For example, if a pure iron should give a diagram 50 inches long, while the length of a diagram of a piece of steel were only 15 inches, some difference in the constitution of the two would be the cause of the modification. Again, following out the values obtained by Mr. Cloud seems to lead to absurdity. Thus, if one-hundredth of one per cent. of iron gives, as in Mr. Cloud's table of values, an elongation of 0.23 of an inch, 100, or pure iron, would yield a diagram of about 23 inches. This is a short diagram, even for steels containing as high as 0.10 per cent. of phosphorus, 0.35 to 0.40 per cent. of manganese, and 0.455 per cent. of carbon. A diagram of carbon alone would be 2954 inches minus—that is, no diagram at all; and this would apply similarly to phosphorus and manganese, both of which have minus signs. The values for these constituents, he urged, had been arrived at by regarding them in a parallel way with iron, by which process the values of some of the principal substances would not give a diagram at all—they would give a negative diagram. He repeated his doubts as to the equatability between the unit of measurement of the diagram and the contents expressed in hundredths of one per cent.

Dr. R. W. Raymond said that Dr. Dudley's arithmetical objection that there was nothing equatable there, would be deprived of much of its force if the author of the paper had worded the equations a little more carefully. The figures need not be changed at all, but, instead of saying u stands for iron, he might begin by basing the whole calculation on the assumption that, if a given weight of a given ingredient has a certain measurable quantity of effect, then twice that weight would have twice that effect. It is assumed, first of all, that if a certain amount of iron possesses a certain effect, twice that ingredient will produce twice that effect; u stands for the effect of a unit of iron, and v for the effect of a unit of the next element, so that, if these units are added together, a sum is arrived at which is, in some way or other, a function of the measurement of the diagram, and Dr. Dudley would be entirely disarmed if the letter m were written before each unknown quantity. The latter would appear in every part of the table and would not hurt anything. Dr. Dudley's criticism, he thought, applied to the way of reading the equation. It was a question, not of resultants, but of relations. Dr. Raymond said that the only point to which he wished to reply was the statement that there was not a fair basis

for calculation, simply because there was no arithmetical equatability.

Prof. T. Egleston: Since it is clearly proven that the chemist is incapable of solving this problem, I think he ought to surrender it to the physicist. I cited, at the Baltimore meeting, analyses of irons and steels that were chemically faultless, and yet they were physically worthless. I also cited rails which I examined in 1873, which were up to the contract stipulations with regard to mechanical tests, and yet were worthless as rails, having been broken in the track and failing to show uniformity of physical condition for more than two feet in the length of the rail. I think it is fully time that the physicist took the place of the chemist.

Dr. Raymond replied that chemical and physical investigations are inseparably combined, and that an indispensable requirement for samples which are to be chemically investigated was that they be manipulated exactly alike physically and mechanically. The absence of such uniformity introduced some uncertainty into Mr. Cloud's deduction, who had frankly admitted that to be the greatest fault connected with his attempt to supply a new method of investigation.

Mr. W. R. Jones, of the Edgar Thomson Steel Works.—Having already taken part in the discussion at the Baltimore meeting as to what constitutes a good steel rail, I did not intend to add more to the discussion at this meeting, but the recent interest excited by the purchase by Mr. Vanderbilt of steel rails in Europe, and the statements that have been made that English steel rails are superior to the American, seem to call for some comment.

A brief *résumé* of the causes that led to the investigations of Dr. Dudley in the interest of the Pennsylvania Railroad may here be in order. It was evident that certain steel rails had not given good results; naturally enough these rails were tested chemically and physically to ascertain if they differed from those that gave good results, and after testing twenty-five specimens, Dr. Dudley prepared and submitted his views based on these tests to the proper officers of his company. All the prominent officers connected with the manufacture of steel rails for the Pennsylvania Railroad Company were invited to Altoona to discuss Dr. Dudley's paper and to suggest improvements or alterations. In this matter the Pennsylvania Railroad Company has displayed good judgment and a laudable desire to get at the true facts of the case. I am sorry to say that the manufacturers did not meet the Pennsylvania Railroad Company in the

same spirit, and after spending an afternoon in a very reserved discussion of the matter, which was entirely unsatisfactory to all concerned, Mr. William P. Shinn recommended that Dr. Dudley present the paper before the next meeting of this Institute, which would result in a full discussion. So far this has only been partly realized, as but two Bessemer steel works have so far been represented in the discussion.

At the Baltimore meeting I stated that there was no question but that Dr. Dudley's formula would make a good rail, and that this fact could not be controverted, as the rails had been fully tested in the track. Since the Baltimore meeting I have discovered the fact that Dr. Dudley allows the manufacturer to use his own judgment with regard to copper and sulphur, which renders his formula, in my opinion, absolutely worthless. We have, I think, been directing our attention too closely to phosphorus, and have neglected the influence of the more important elements, sulphur and copper.

I have here three pieces of a steel rail taken out of the track of the Pennsylvania Railroad, broken for convenience in transportation. This rail has been out of the track for three years, and as it was made in 1872, we presume that it was in the track about three years. You will notice that it is not homogeneous, as you will find that large pieces have fallen out while in service. Now the question is, what has made this a bad rail? It is the excess of both sulphur and copper, and the lack of a proper amount of manganese to neutralize their bad effects.

Here I show you a section of an ingot containing thirty-two of Dr. Dudley's phosphorus units, being, therefore, very near to his formula. It contains:

		P. U.
Phosphorus,	0.111	11 1
Carbon,	0.260	8.7
Manganese,	0.560	11.2
Silicon,	0.022	1.1
Total phosphorus units,		32.1

This ingot, you will notice, is remarkable for its large cells or honeycombs, and that it would make a good serviceable steel rail I do not believe. I think that a rail made from it would in service show about the same results as the three pieces exhibited.

The first thing, in my opinion, toward making a good serviceable steel rail is to make a sound ingot, free from porosity, sponginess, or honeycombs, and as hard as is compatible with safety.

The two pieces of ingots that I now exhibit (which, as you see, are free from cells or porosity) are fair samples of such rail steel. These are not prepared ingots. We break ingots frequently to examine their structure and to see if they are free from porosity. When we commenced operations at the Edgar Thomson Steel Works it was thought we had embodied all the modern improvements. A few short months developed the fact that there was room for improvement. The improvements made I will not attempt to enumerate; suffice for me to say, that after they were made instead of the Bessemer process being the uncertain thing it was, it is now a very reliable process for manufacturing steel containing carbon as high as 0.70, and as low as 0.04.

The success of the Edgar Thomson Steel Works is due first, to the excellency of the machinery; second, to system in method of working; and third, to closely watching the machinery and not allowing it to deteriorate. All the superintendents and workmen are instructed that quality is the first desideratum, quantity the second. We run steadily and strongly, the output being between 2000 and 2300 tons of ingots weekly. No efforts are made at spurts, which are so destructive to machinery. This system will, I firmly believe, give the rails made by this company a reputation equal to the very best.

All the steel is subject to compression by steam, of the patent for which this company is the exclusive owner. As certain parties have claimed that compression by steam has no merits, I will state a few experiments we have made to test its merits. We have filled a mould to within eight inches of the top, and allowed it to cool of itself. On the next ingot which was poured to the same height the cover was clamped and subjected to a steam pressure of 150 pounds to square inch. On removing the two ingots we found that the first one poured without the compression had swollen and filled the mould entirely, while the one subjected to the pressure had been forced down from one and three-quarters to two inches, being a difference in vertical height of ingot of say ten inches. The great benefit of this compression is that we are free from the great evil of split-end rails. After the ingot is sufficiently cooled to bear transportation it is taken to the heating furnaces. Ninety-nine per cent. of all ingots cast are rolled before they are allowed to cool, and ninety-nine per cent. of rails made are rolled from hot blooms direct from the blooming mill, being reheated before they are rolled.

I have thus briefly outlined our practice. That some bad rails

have been made by American works there can be no doubt. The system known as "bottom cast" has done fearfully bad work, yet to-day this system has advocates who think it superior to the "top" cast. We at first used the bottom cast. After succeeding in casting ingots with holes clear through them, and rolling a thirty-foot rail split from end to end, I deemed it best to discard it; and being one of the quartet of patentees, I certainly can be credited with being honest in my condemnation of it. The bottom cast (if two-rail ingots are cast) renders one end of each rail liable to split ends—the top end from piping and the lower end from bleeding. Bottom casting has made a fearful bad record for American rails.

To return to Dr. Dudley's formula. The idea that so many have imbibed that this is the only correct formula, must be dissipated. The very rails that Dr. Dudley has classified as being good rails could have been made far better. The reputation of John Brown steel rails no one questions. I append an analysis of John Brown steel that has given remarkably good service. A rail in the Baltimore and Ohio track eleven and one-half years, with a tonnage of 55,000,000 tons, shows

		P.U.
Phosphorus,	0.069	6 9
Carbon,	0.360	12.0
Manganese,	1.404	28.1
Silicon,	0 124	6 2
Sulphur,	0.071	. . .
		53 2

Other samples of this steel show

Manganese,	from 0.774 to 1.079
Silicon,	from 0 022 to 0 187
Carbon,	from 0 026 to 0.590
Phosphorus,	from 0.104 to 0.500

All these rails having done good service. After years of close observation, I would not attempt to prescribe any particular formula unless I was conversant with the irons to be used, and at the Edgar Thomson Works the formula is changed to meet the characteristics of the irons used.

I exhibit two specimens of steel rails of the following composition:

Phosphorus,	0.223
Carbon,	0.120
Silicon,	0 025
Manganese,	0.387

Phosphorus units, 35.29.

Test by Thurston machine:

Tensile strength at elastic limit,	39,750 pounds.
Ultimate tensile strength,	78,892 "
Percentage of elongation,	37.9
Angle of torsion,	1.74 degrees.

This is a near approach to the Terrenoire formula. Since the Baltimore meeting I have had some correspondence with Colonel De Funiak, Chief Engineer of the Louisville and Nashville Railroad, who says the Terrenoire rails have given the very best results and are made as surmised, high in phosphorus, low in carbon, and high in manganese. I have also learned from one of the inspectors of the Pennsylvania Railroad Company that the Terrenoire steel on the New Jersey Division has shown very good results. All of which goes to show that good steel can be made from quite a variety of formulæ, and that phosphorus in rails is not the bad fellow we have claimed him to be, and that while we have condemned phosphorus only, we have entirely overlooked the worse elements, sulphur and copper.

The bent specimen was subjected to a drop test of thirty-six feet, ram weighing 1620 pounds, with bearings three feet from centre to centre. The piece had to be reversed before it could be broken, in order to show the fracture of the steel, which you will notice is very fine.

As a fair specimen of some of the specifications made by railroad companies, I cite that of a prominent railroad company for 1879, which simply says—carbon between 30 and 50. Now I would undertake to fill this order, and guarantee every rail to break in service and the greater part of them before they got into service, and yet by the terms of the contract and their specifications, they would be compelled to accept every rail made. Since Dr. Dudley's paper has been published, quite a number of railroad engineers have based their specifications on his (Dr. Dudley's) formula, but are always very careful to claim that it is the result of their own investigations; and for the benefit of this particular class, I tell them that they will get for their trouble very bad rails. The very makers of some of the bad rails complained of by the Pennsylvania Railroad Company use this argument for their justification. They say, "We are not responsible; they ask us for a guarantee and specify how rails shall be made. We are not to blame; let them allow us to make the rails as we deem best, and we will guarantee better results."

An official of a prominent road prescribed for us Dr. Dudley's

formula of thirty phosphorus units, after getting some 10,000 tons of rails from another mill, with the following as a sample analysis:

Phosphorus,	0.22
Carbon,	0.50
Manganese,	1.20

A member of this Institute (Mr. W. A. Sweet) has recently published a letter in which he says that there is no doubt that English steel rails are better for wear than the American, and gives as the reason that the rail is rolled colder in the finishing passes. I quote from his letter: "The superiority of wear is not the result of any known reason on the part of the English makers, but from an actual unpremeditated and unstudied fact—simply this: they roll the rail colder when they finish it. This I have told to nearly all the railmakers in the country and they heeded it not. Now they have the fact brought to them in such a way that they must heed it. . . . Your readers will ask why the English roll their rails colder; and I answer, for the simple reason that they use a two-high train of rolls, and they cannot get out a rail as hot as they do in this country. I have argued for at least six years that Bessemer steel is not well and properly manipulated for any such wear unless rolled cold enough to set the scale. All rollers of steel know well what this term means. Were I a steel-rail maker I would use for the last three passes a polished chilled roll, and roll so cold that the rail would look smooth and well polished. This would put the steel in proper condition to wear; and were I a railroad manager, I would not ask a guarantee of twelve years, but a guarantee of so many thousand wheels tonnage, and I would not have any rails laid down under my management unless they were rolled as I have herein specified."

Now I do not believe that any amount of cold rolling, with chilled or unchilled rolls, would make a good rail of the specimen I exhibit. Nor can I see any necessity for the luxury of chilled rolls, particularly when we consider the tendency of modern rail designers for wide thin flanges. Nor should Mr. Sweet have made so sweeping a charge against all the steel-rail makers. Mr. Sweet can be shown letters from the officials of the New York Central Railroad, in which they state that Troy rails have given equally as good results as the very best foreign rails. I myself have seen Troy rails that have been in a sharp curve on the New London and Northern Railroad for over five years, that show but little wear and looked good for at

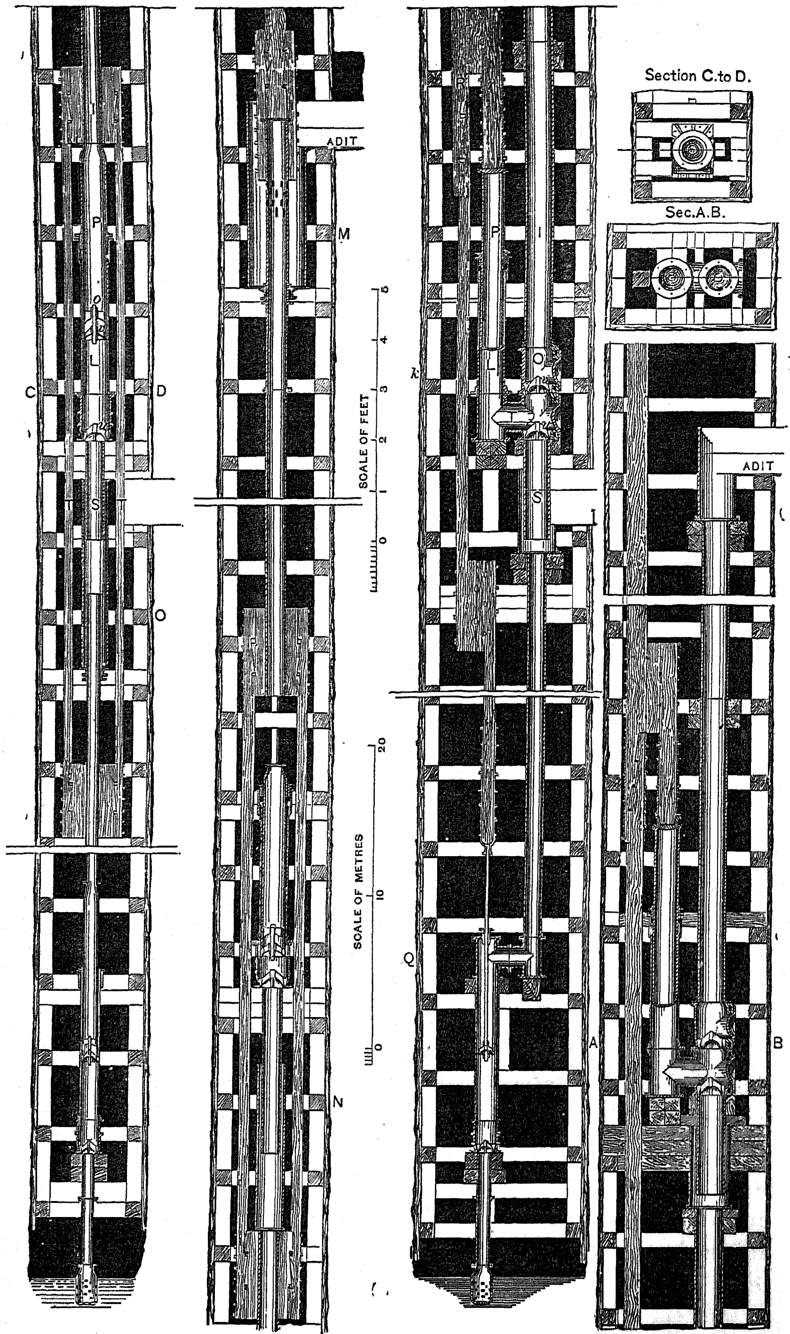
least ten years more. The Edgar Thomson has rails laid for three years on a heavy grade on the Pennsylvania Railroad, subjected to the heaviest tonnage. I had these rails carefully measured by three competent persons accustomed to careful measuring, on last Monday, and they find the amount of wear to be $\frac{5}{100}$ths of an inch, and it certainly looks as if these rails would last for over fifteen years.

Mr. Sweet's assertion that the English roll colder than we do, I do not think can be sustained. It is an error to suppose that because they use two-high rolls that they roll slower; on the contrary, they roll faster, as Mr. Holley can fully explain.

Nor do I believe that if Mr. Sweet owned or controlled a steel-rail mill that he would long continue to use chilled rolls, the effect of which would be to seriously decrease the amount of elongation under a tensile strain.

Dr. Dudley, in reply to Captain Jones, called attention to the fact that he had in his original paper recommended that no specification be prescribed for sulphur, because a piece of steel containing too much of that substance would be red-short, and could be rolled only with difficulty. He believed that Captain Jones would agree with him when he stated that steel containing not over 0.30 per cent. of carbon, not over 0.10 per cent. of phosphorus, and not more than 0.34 per cent. of manganese, could not be rolled if too high in sulphur. When the paper came to be published, copper was included with the sulphur, as he believed that the former acted much like the latter by making the metal red-short. He would not express an opinion on the subject of the effect of sulphur and copper on the wearing power of steel; he thought that it affected the manufacturer more than the buyer. Captain Jones, he said, did him full justice in stating that he (Dr. Dudley) had brought the matter forward with the intention of trying to get better steel. He should be more than gratified if they succeeded ultimately in advancing their knowledge of steel as regards its wearing qualities, no matter whether the formula he had given did or did not make the best rail.

FIG. 1.



MOVING PIPE PUMP

CORNISH PUMP

AN IMPROVED SYSTEM OF CORNISH PITWORK.

BY ELLSWORTH DAGGETT, MINING ENGINEER, SALT LAKE CITY, UTAH.

THE system of pitwork used with the Cornish pumping engine, and which, for want of a better name, we may call the Cornish system of pitwork, consists essentially of a series of plunger-pumps, situated one below the other, in a shaft or incline, the plungers being operated by a rod called the pitman or pump-rod, which must, therefore, extend from the motor at the surface to the lowest pump.

The subject will be best understood by reference to Fig. 1, in which *k* may be taken as typical of the ordinary plunger-pump, in which S is the suction pipe, *u*, the lower valve situated in the H piece T; *v*, the upper valve situated in the valve-box O; I, the mounting-pipe or column through which the water is carried to the surface; L, the plunger-barrel, and P, the plunger working through the stuffing-box, and actuated by the branch rod *r*, which is attached to the main pump-rod, R. The action of the pump is as follows: On the ascent of the plunger, acting now by suction, the water flows from the reservoir through the valve *u*, into the barrel, L. On the descent of the plunger, the water is forced up through the valve, *v*, which acts merely as a check valve, into the pipe, I, through which it is, at each successive down-stroke of the plunger, forced upward until it reaches the next reservoir, from which it is taken by another similar pump, and so on until it reaches the surface or drain-tunnel. The pump is obviously only single-acting, *i. e.*, it acts only during one-half of its entire stroke.

The weight of water raised during the up-stroke of the plunger is equal to the weight of a column of water whose base is the area of the plunger and whose height is the mean distance between the plunger-face and the surface of water in the lower reservoir, a distance not usually exceeding 12 or 15 feet, and which obviously cannot under the most favorable circumstances, exceed 25 or 30 feet.

The water forced up during the down-stroke is equal to the weight of a column of water whose base equals the area of the plunger and whose height equals the length of the mounting column above the mean position of the plunger-face, say from 150 to 300 feet. Almost all of the useful work, therefore, is done on the de-

scent of the pump-rod.* The bottom pump of the Cornish system has usually consisted of some form of a lift pump like that shown in sketch (see Q), known as the Jack head pump, or better, a simple pump barrel surmounted by a pipe large enough to allow the bucket to be withdrawn and to contain the pump-rod, thus permitting the replacement of the bucket and even of the lower valve when the pump is submerged. Usually but one lift at the bottom, and that the shortest lift of all, is operated by the lift pump, and in some cases the plunger pump first described has taken the place even for the bottom pump.

The height of lift proper for a plunger pump depends on the duration of the leather used in packing the valves, these latter requiring renewal oftener with a high than with a low column of water. It is really then a compromise between too frequent renewals of the valves on the one hand, and too great multiplicity of pumps and consequent great cost on the other. They are usually placed from 200 to 300 feet apart.

Pump-rods.—The main pump-rod, which must of course extend from the motor to the lowest pump, is usually in this country made of wooden rods spliced in various ways, the joints and attachments being strengthened by iron straps and bolts. The weight of the pump-rod is usually considerably in excess of that of the column of water raised. The proportion existing between the weight of the column of water raised and the weight of the pump-rod is extremely various; the rod weighing from 1 to 3 times the column of water.

Balance-bobs and Counterbalances.—The object of the balance-bobs found in the Cornish pump is twofold: 1st, to counterbalance the excessive weight of the rod, so that the weight of the latter when left free to fall shall be just sufficient to overcome the friction of the plunger, to open the valves and to give to the ascending column of water the required velocity; and 2d, to equalize the actual work performed by the engine during the up and down stroke. The latter consideration has influence only when double acting or geared engines are in use, and is usually accomplished to a great extent by the large bob at the surface, which also serves as a convenient means of changing the direction of the power from horizontal to vertical.

* In some cases, indeed in the more modern and perfect Cornish pump, the reservoir is so situated that the surface of the water in it is as high or higher than the mean position of the plunger face, in which case the entire useful effect is during the down stroke.

Although this pitwork has been modified to suit varying circumstances; although innumerable forms of pumps have been used; although the pump-rod has consisted of various forms of wood and iron, from the trunks of trees down to wire rope; yet the principle of Cornish pitwork has not changed materially for several generations.

One feature of the system, perhaps as characteristic as any, is the use of the pump-rod not merely to convey power from the motor to the pumps, but to receive and store it, as it were, to give it out again afterwards to do the work that should be done by the motor direct. Thus the almost universal practice is to lift the pump-rod and attachments, which on their descent by gravity force up the water.

This indirectness in the application of the power to the work is a false mechanical principle, and with our present facilities for making pump-rods of maximum strength and rigidity with minimum of weight, its continuance is utterably inexcusable. More especially is this true with the modern engine, taking steam at both ends of the cylinder, or with the geared engine, in which the power of the engine is transmitted to the pump-rod by means of a pinion and spur wheel, both these cases requiring a heavy counterbalance to equalize the work on the up and down stroke.

The fact is, that the Cornish pitwork, such as described above, belongs to and with the Cornish pumping engine. It is, so to speak, one of the unfortunate necessities of the Cornish pumping system. The Cornish pumping engine is the redeeming feature which alone should render the pitwork advisable.

Now, while we have discarded the Cornish engine, which on account of its great size and cost is not adapted to our use, we have clung to the heavy and cumbersome form of rod, with a persistence worthy of a better cause, and have almost entirely neglected the advantages arising from the use of better material in a more suitable form—advantages which have been turned to account in almost every other structure, from the umbrella to the marine engine.

We have disjointed a system which in its entirety is, within certain limits, most admirable, and attempted to use its worst feature in a new place for which it is not adapted.*

* This applies particularly to England and the United States, notable advance, especially in the adoption of iron-riveted tubes and truss-formed pump-rods, having been made in Europe. Descriptions and drawings of two forms of iron pump-rods, now in use, may be seen in Burat's "Cours d'Exploitation des

Wrought-Iron Pipe as Pump-rod.—Since in the new system of pitwork here proposed, use is made of wrought-iron tubing, as a substitute for the ordinary wooden rods, it may be proper to investigate its capacity to act at once as a mounting pipe for the water and as a pump-rod. The duties required of it in this double rôle may be briefly stated as follows: With a minimum weight it should possess a maximum of 1st, strength to resist bursting pressure; 2d, tensile strength along the axis of the pipe; 3d, capacity to resist thrust or to act as a pillar; 4th, inflexibility, in which I include absence of lost motion; and 5th, it should possess a joint that can be made and disconnected with reasonable facility, which is water-tight and which in no way impairs the strength of the pipe.

1st. As to the strength to resist bursting pressure, it is hardly necessary to show that a wrought-iron lap-welded tube is the best material except steel, and that it possesses the most suitable form. Such tubes are now made of any required thickness, and as large as 16 inches in diameter, and can doubtless be made 20 inches or more, if required.

2d. The tensile strength obviously depends on the area of the cross-section or metal-area. In this respect, an iron pipe is about on an equality (in proportion to its weight) with the wooden pump-rods, as ordinarily constructed. Thus if we assume the ultimate tensile strength of pine at 10,000 pounds per square inch, and of wrought iron at 50,000 pounds per square inch, we have one inch of iron equal in tensile strength to five inches of pine. Now the specific gravity of wet pine is about .75, and of iron, 7.7; hence, weight for weight, the pine alone has about double the tensile strength of wrought-iron. But the usual method of splicing with straps and bolts increases, on an average, the weight of the wooden rods not less than 80 per cent., and the joint ordinarily used decreases the strength by 25 per cent.; while iron pipes may be joined with an average increase of 20 per cent. in weight and with no loss of

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Mines," edition of 1871, page 385, Plate 49, Figs 1 and 2 of Atlas. Also a more complete description of the same, together with details of other forms in use and proposed, in the Preussischen Zeitschrift für das Berg-Hütten und Salinen-Wesen, vol. 17, Part 3, page 315–319, Plate 22.

The most rigid of the forms thus far used or proposed, approach in the form of their cross section, the square tube □, the +, or the H; and with metal-area and all other conditions equal, have, when used as a pump-rod, only three-fourths or less than three-fourths, the rigidity or thrust-carrying capacity of lap-welded tubing.

tensile strength: hence the ultimate strength of wooden rods with their joints is about the same as that of wrought-iron pipe.

3d. It is in its capacity to resist thrust that the great advantage of the lap-welded tube lies. To illustrate this, I have prepared the following table, in which the weight per foot of pine timber 50 feet long, jointed in the usual way, is compared with the weight per foot of a column of same length, composed of wrought-iron tubes, jointed as here described and sustaining the *same breaking load:*

Pine Timber 50 Feet long, Jointed in the usual manner.			50 Feet Lap-welded Tubes with Joints. Sustaining same Load.†		
Inches square.	Breaking load,* tons of 2000 pounds.	Weight per foot. Joints included.	Diameter.	Weight per foot. Joints included.	Ratio.
9	11 2	45 lbs.	9	6. lbs.	0 13
10	16.6	56 "	10	8. "	0.14
11	23 5	66 "	11	10.5 "	0.16
12	32.7	81 "	12	13 2 "	0 17
14	58.	109 "	14	20.8 "	0 19
16	94.	144 "	16	30.8 "	0.21
18	146.	182 "	18	44 "	0.24

* Calculated by C. S. Smith's formula, in which the breaking load of pine, in short blocks, is taken at 5000 pounds.

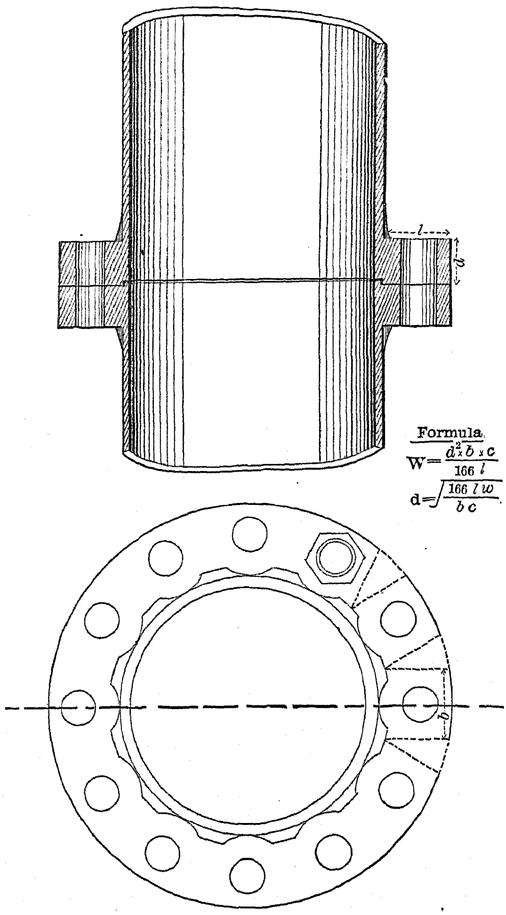
† Calculated by Gordon's formula, in which the crushing load, per square inch of wrought iron, in short blocks, is taken at 36,000 pounds.

Or in words, a given thrust will be conveyed by a wrought-iron tube 50 feet long, whose weight is only .13 to .25 of the weight of a pine timber of same length, capable of conveying the same thrust; or again, an iron tube used as a pump-rod, guided accurately every 50 feet, will convey from 4 to 7.5 times the thrust conveyed by a pine pump-rod of the same weight with guides at the same distance.

4th. It is evident that if any lost motion exists in an iron tube it must be in the joint. The form of joint adopted as best adapted to the work is shown on Fig. 2. It consists of a flange of wrought iron welded on to the pipe, which at the end is slightly upset, and is proportioned as follows: As many bolts are used to connect two lengths of pipe as the circumference of the flange allows; the diameter of each bolt being such that the net cross-section, *i. e.*, the cross-section inside the threads, is at least equal to the cross-section of iron in the pipe, divided by the number of bolts. In other words, the sum of the net areas of the iron in the bolts must at least equal the net area of iron in the pipe. This is, of course, supposing the quality of the iron to be the same. The width of the flange, *i. e.*, the distance from the outside of the pipe to the outside of the flange, is only sufficient to allow the nut to turn. The thickness of the flange is determined

by the formula for beams fixed at one end and uniformly loaded, in which *b* (the breadth), equals the outer circumference of the pipe divided by the number of bolts, *d* (depth), equals the thickness of the single flange, and *l* (length), equals the width of the flange.

FIG. 2.



The joint is made water-tight by placing a ring of copper wire in the recess shown. The face and back of each flange must be turned true in the lathe, so that when the two parts of a joint are brought together by the bolts there is an actual contact of the iron. The weight of such a joint would be 10 to 25 per cent. of the weight of the pipe, depending on the size and metal-area of the pipe and the length of the section.

In making the joint, the two flanges should be pressed together by the bolts with a force as great as can ever come upon the pipe. If this is done, it is obvious that no strain less than the maximum for which the pipe is calculated can possibly open the joint made by the copper wire, or to the slightest extent separate the two flanges, thus creating lost motion and leakage.

It is believed that a very much less depth of flange than that found by the formula will answer, owing to the bracing given to the joint by the actual contact of the two faces, and also to the fact that the beams, as calculated by the formula, are then regarded as separate, while in reality they are united, forming the flange. It is probable that the strain approaches the shearing strain.*

Moving-pipe Pump.—In endeavoring to overcome some of the principal objections to the old Cornish pump, to make it more like a modern machine, to at once cheapen and simplify its construction, and better adapt it to the modern high-pressure steam-engine, I have devised a system of pumping which, though not entirely new in its details, is, I believe, well adapted to the draining of deep mines.

This pump, which may be called the Moving-pipe Pump, is shown at C D, Fig. 1, where S is the suction pipe, *u* the lower valve, situated in the bottom of the valve-box, *v* the upper valve, situated in the bottom of the hollow plunger P, and moving with it, L the pump barrel, surmounted by a stuffing-box, and I the mounting-pipe, bolted to and actuating the plunger. It is obvious here that the pipe serves at once as a mounting-pipe and pump-rod. The action of the pump when pump and pipe are full of water is as follows: On the up-stroke of the pipe and plunger, the water is drawn up through the suction-pipe and lower valve into the pump barrel, L, and as the upper valve, V, is shut, the water above it in the plunger and pipe is lifted with the pipe. On the down-stroke, the lower valve being closed, the water in the pump barrel, L, is forced up into the pipe, occupying just so much greater length of the pipe as the area of the circle of the outside of the plunger is greater than the area of the circle of the inside of the pipe; thus if we suppose the area of the outer circle of the plunger to be double that of the inside circle of the pipe, the water displaced in the pump barrel will occupy a length of the pipe equal to double the length

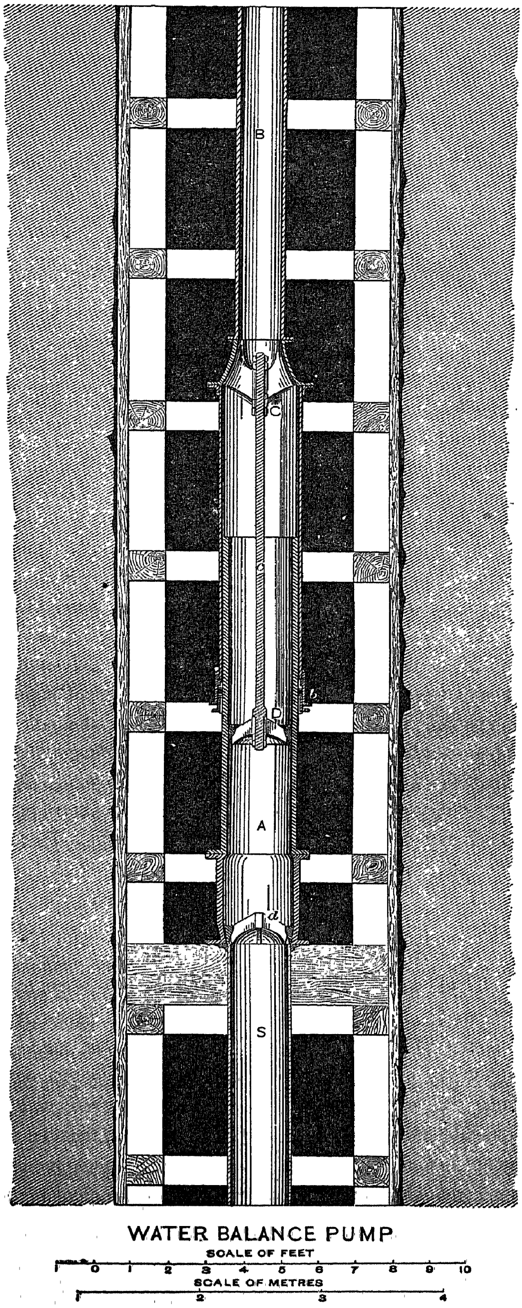
* As this structure is here used to resist a new strain, its exact proportion, in order that the joint should be as strong as the pipe, should be determined by experiment in the shop. Especially is this true of the total bolt-area, which would probably have to be slightly in excess of the metal-area of the pipe.

of the stroke; or since the pipe descends the length of the stroke while the water is going up in the pipe two lengths, the actual rise of the water is equal to one length of the stroke. The water is, therefore, raised both during the up and down stroke of the pipe, hence the pump is *double acting*, in the sense that the useful effect is divided between the up and down strokes, instead of being exerted entirely on the down stroke.

In pumps of this kind the total weight raised on the up stroke is equal to the weight of the pipe and its attachments plus the weight of the water in the pipe. On the down stroke the weight raised is that of a column of water whose base is the difference between the areas of the pipe and the plunger, and whose height is the mean distance from the upper valve to the level of the discharge. If, as with some Cornish pumps, the supply-reservoir be situated as high or a little higher than the mean position of the plunger-face, the useful effect becomes practically divided between the up and the down stroke in proportion to the area of the pipe, and the area of the plunger less the area of the pipe. It is this division or distribution of the useful effect between the two parts of the stroke which it is thought will enable the moving-pipe pump, with less weight of pitwork than the Cornish pump, without any pump-rod other than the pipe itself, and with corresponding economy in construction and space, to equal or excel the work of the Cornish pump. This does not apply to economy in working, but only to the amount of work.

Theoretically, this distribution of the useful effect would, if carried far enough, enable us with this form of the moving-pipe pump to dispense entirely with the counterbalance, leaving at the surface the same amount of actual work on the engine or other power during both parts of the stroke. This very desirable equality of actual work by the engine would obviously be attained when the weight of water raised on the down stroke is equal to twice the weight of the pipe and its attachments plus the weight of the pipe full of water. If, to make this plain by example, we suppose the weight of the pipe with its attachments to be equal to the weight of the column of water in the pipe, then on the up stroke the engine must raise pipe and water, and on the down stroke exert the same power as a thrust, aided moreover by the descent by gravity of the pipe; the useful effect, or water raised, being therefore three times that of the up stroke. But as this water must be forced up through the pipe, its actual velocity will be three times that of the pipe, and as the pipe

FIG. 3.



WATER BALANCE PUMP

is at the same time descending around the water, its velocity in relation to the pipe will be four times that of the pipe itself. It is this increased velocity and the consequent friction, together with the great number of guides necessitated by the great thrust conveyed by the pipe, which limits the extension of this principle to meet any case. In the case just cited, the area of the plunger would have to be four times that of the pipe. It is evident that with this pump the utmost lightness of pipe compatible with sufficient strength is desirable.

As is well understood, one of the chief difficulties of the Cornish pump when of large dimensions, and operating at great depth, is the enormous mass of the pump-rods, counterbalances, and attachments to be set in motion at each stroke, necessitating a very low velocity.

In the moving-pipe pump the difficulty would at first seem to be increased, since on the up stroke there must be lifted not only the pipe but the water contained in it, thus increasing the work, where the work is already the heaviest. But here it must be remembered that the column raised is only half, or less than half, the area and weight of the column in the corresponding Cornish pump, and that, as there is no object to reach by making rods heavy, they may, by using a peculiar form of rod, be made so light that the total weight to be raised will be the same, or less, than in the Cornish pump.

Water-balance Pump.—It will be remembered that, in the original Cornish pumping-engine, the entire power of the engine is exerted on the up stroke in raising the rod and its attachments, and that this is all the work done by the engine during the entire stroke. Now with the double-acting or geared engine, generally used in this country, the power is exerted at all parts of the stroke, and the work must be equalized by the use of a balance-bob, often of great size and cost. The question naturally arises cannot this be accomplished in the mechanism of the pump itself? There are obviously two methods of effecting this: 1st, by increasing the actual work on the down stroke; 2d, by decreasing the work on the up stroke.

The first of these methods is applied in the pump already described, but is of only limited application. It is proposed to apply the second in a form of pump called the water-balance Pump, which is also a moving-pipe pump. One form of this pump is shown on Fig. 3, in which A represents the pump-barrel, which is shown to be of considerable thickness, for the reason that it is desirable to have a greater exterior diameter of the barrel than would be attained if the metal were no thicker than required in an ordinary pump-

barrel of the same interior diameter. Or it may be cast in two parts, leaving a space between, like a steam-jacketed cylinder.

C represents one form of an auxiliary chamber, by means of which the "water-balance" is effected. It is connected by means of its dome and flanges to the lower end of the reciprocating pipe, B. It has an interior diameter greater than the exterior diameter of the pump-barrel, and the sliding joint between them is guarded by a stuffing-box at *b*. The valve D is attached to the lower end of the rod *c*, which, in turn, at its upper end is centrally secured to the interior of the dome of the auxiliary chamber C, in a manner well known. The usual lower valve is shown at *d*.

It will be readily seen that, as the pipe B is lifted, water will be drawn through the lower valve into the pump-barrel; and, also, that the water in the upper portion of the pump-barrel is lifted by the moving valve. As the auxiliary chamber C is lifted, its cubic capacity is enlarged by the withdrawal of the thick pump-barrel A, so that the water above the valve D, instead of being raised with the pipe B, simply occupies the annular space then left vacant. In other words, the column of water in pipe B (having its base in a chamber which is enlarged as the pipe rises) induces pressure within the chamber, which, when exerted upon its dome, contributes to the lift of the pipe and lessens the power otherwise requisite to raise it; and it will also be seen, if the chamber be properly proportioned with reference to the pump-barrel and the pipe B, that the weight of the pipe may be offset or balanced by the column of water within the pipe. As the pipe B descends, the upper valve opens and water passes from the lower portion of the pump-barrel, the cubic capacity of the auxiliary chamber meantime decreasing, and therefore at each descent of the pipe as much water is discharged therefrom at its top as is taken into the pump-barrel through its lower valve, which is all that any pump could discharge which operates in forcing water only during the downward movement of its piston.

When the area of the annular space formed by the end of the pump-barrel is greater than the area of the pipe, so that the column of water in the pipe by its descent assists in lifting the pipe, the machine becomes analogous to the ordinary Cornish plunger-pump, with an ordinary water-balance attachment, instead of a balance-bob; the difference being that a single pipe serves at once as a pump rod, as mounting column and as water-balance column.

In the water-balance pump, as with the moving-pipe pump, the limit of the application of the principle is reached, 1st, when the

velocity of the water on the down stroke becomes too great; 2d, when the thrust required exceeds the adopted limit of safety. In regard to the first of these points, I believe that, when any corresponding advantages are to be gained, the velocity of the water in the mounting column may be very much increased. This is specially true of pumps operating at great depths, where the mass set in motion and stopped twice during each stroke, must be with any form of pitwork very great, and where in consequence, the motion of the pump-rod must be slow.

As illustrative of this, let us suppose a series of 12-inch pumps of the Cornish system, operating to a depth of 2000 feet. The maximum speed of such a pump could not well exceed 160 feet per minute, and the ordinary speed would be probably less than 120 feet per minute, and the loss incident to the friction of the water flowing up the mounting column would be, for the last-named speed, but 16-100 of one per cent. of the useful effect. Now the loss from friction of the same amount of water flowing up a mounting column, one-third the area, or say 7 inches diameter, and with three times the velocity, or 360 feet per minute, would be 2.06 per cent. of the useful effect, showing an advantage of only 1.9 per cent. in favor of the large pipe in this extreme case. In short, the practice of making the mounting-pipe of the ordinary Cornish pump as large or larger than the plunger, is, I take it, traditional, and may be departed from if there is anything gained by the departure.

With regard to the second point, if we suppose the pump to be operated by a double-acting or geared engine, or any other motor in which the power is exerted continually, it is obvious that with no counterbalancing arrangement the thrust communicated to the pipe at the top and conveyed by it, should equal the tensile strain at the same place; but the capacity of the moving pipe when guided, say every 40 feet, to convey thrust, is only 1-3 to 7-12 (depending on its size), of its ability to resist tensile strain, or in other words, it can lift from 3 times to 12-7 times as great a weight as it can support; and if we make it strong enough to resist all the thrust, it will be two or three times as strong, hence two or three times as heavy as required for the tensile strain.

The safe loads per square inch sustained by wrought-iron columns forty feet high and of different sizes may be shown by the following table, in which 36,000 pounds per square inch is taken as the elastic limit (in short blocks), and in which the allowed strain is taken at one-eighth that amount.

Compressive Strength per square inch, Metal-area of pipe 40 feet long.

8 in. diameter,	2038 lbs.
9 " "	2206 "
10 " "	2550 "
11 " "	2732 "
12 " "	2934 "
13 " "	3192 "
14 " "	3248 "
15 " "	3360 "
16 " "	3472 "

It would seem therefore, that the use of the counterbalance principle should only go far enough to bring the thrust per square inch of metal-area of the pipe up to its adopted maximum (say one-eighth the crushing load); and that a minimum of weight in the whole structure will be secured when the strains on the pipe, both thrust and tensile strain, are up to the limit adopted, leaving the final exact equalizing of the work of the motor to be effected by a balance-bob or additional water-balance at the motor.

The extra surface counterbalance required with a properly proportioned water-balance pump to equalize the work at the motor, would be from two-thirds to one-fourth that required for an ordinary Cornish pump of same capacity.

Description of Plates.—Of the accompanying illustrations, Fig. 1 shows side by side a large ordinary Cornish pump and a moving-pipe pump of the same capacity per stroke and same length of stroke.

In the moving-pipe pump the tensile strain is carried from one pipe to the next lower pipe by means of the wooden tie beams T T, strapped to the pipes as shown.

In neither case is any thrust conveyed by the rod or pipe, since they operate entirely by their own weight. The counterbalancing required in each case is nearly the same.

The supposed conditions of work are 500 gallons per minute to be raised from the bottom of the shaft 800 feet to an adit level, and 500 gallons per minute to be raised from a point 200 feet above the bottom 600 feet to the same level.

Some of the dimensions of the pumps are as follows:

		Cornish Pump.	M. P. Pump.
Lowest pump.	Diameter of barrel or plunger,	14 inches.	14 inch.
	Diameter of mounting pipe,	14 "	10 "
	Height of lift,	200 feet.	200 feet.
Second pump from bottom.	Diameter of plunger,	20 inches.	20 inch.
	Diameter of mounting pipe,	20 "	14 "
	Length of lift,	300 feet.	300 feet.

Third pump, from bottom—same as second.

The strain on the various parts of the two pumps is computed with the same factor of safety, and the estimates of cost are made in each case on the same basis, in fact the designs and estimates were made with a view to get at a fair comparison of the two systems under those conditions. The weight of the entire pitwork for the moving-pipe pump is 45 per cent. and the cost 63 per cent. of the weight and cost of the Cornish plant.

A reason for the less weight of the moving-pipe plant, will be seen by an inspection of the two middle pumps, in which it will be noticed that in the moving-pipe pump the heavy H piece, the largest and heaviest casting of the Cornish pump, is entirely dispensed with.

It is expressly stated here that Fig. 1 does not and is not intended to show the best form of either pump. Just this plan would be neither advisable or practicable under many conditions. It is introduced merely for the sake of a direct comparison of the two systems.

Fig. 4 shows an elevation and section of a moving-pipe pump, in which the motion of the upper pipe is conveyed to the lower pipe by the smaller pipes on each side, which are attached to the main pipe by the cast-iron cross-head as shown. The discharge of the lower pipe is here effected in such a manner as to flood or deluge the lower valve of the pump.

Figs. 5 and 6 are plans for a moving-pipe pump, designed to raise 1000 gallons per minute to a height of 1000 feet from the bottom of a shaft to an adit level 1000 feet above, and 40 feet below the engine-room floor.

Fig. 5 is a section and elevation of the upper portion of the pitwork, showing the method of discharging water at the top, the connection with the balance-bob, the cross-head guide, and at the bottom the ordinary guides for the pipe, consisting of four pieces of hard wood, bolted to a timber framework, touching the pipe at four points and adjusted to it by means of keys.

The upper portion of Fig. 6 shows one of the upper three pumps, which are alike in section and elevation.

In this case the motion of the upper pipe, instead of being conveyed on each side of the pump by wooden rods as in Fig. 1, or by smaller pipes as in Fig. 4, is communicated directly and in the same line to the lower pipe, the pump being set on one side. The pump is also different from that already described, in that the upper valve, instead of being attached to the bottom of the plunger, is situated in the left arm of the nozzle or short piece connecting the

FIG. 4.

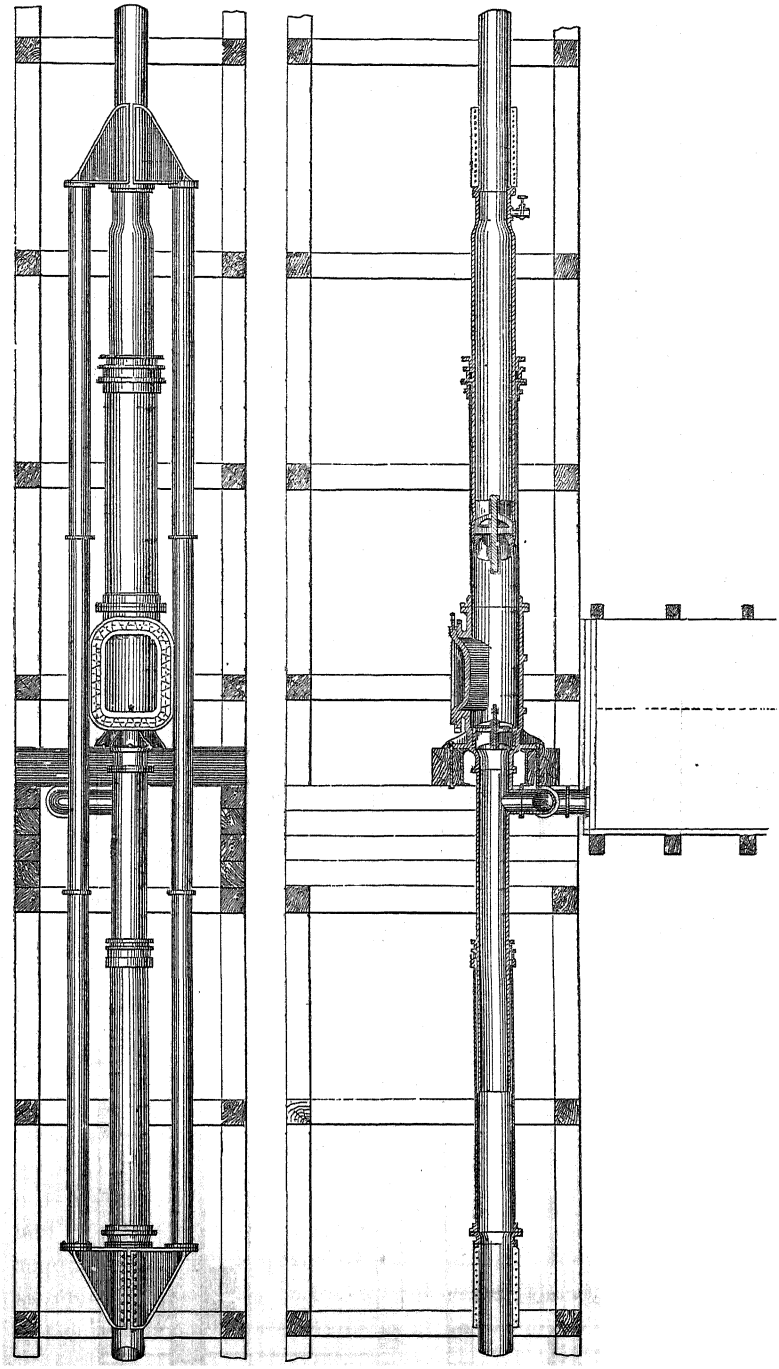
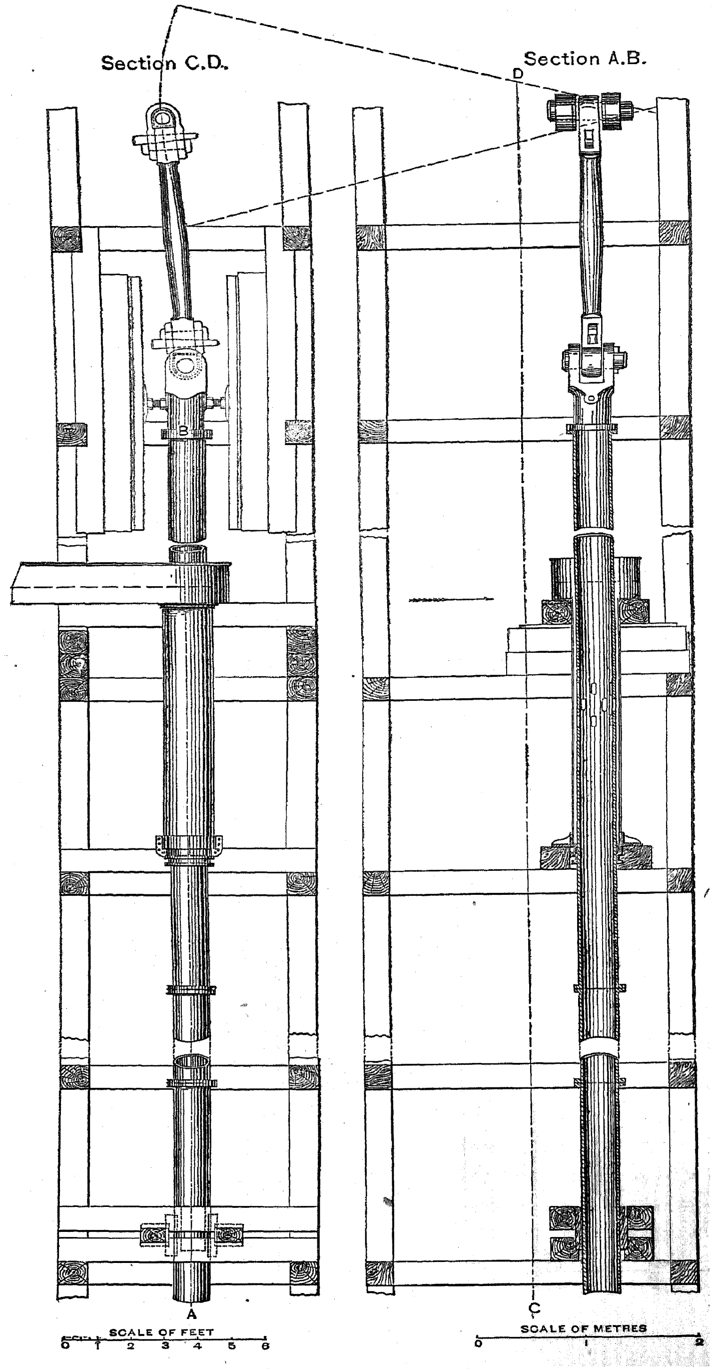


FIG. 5.


Section C.D.
Section A.B.
D
B
A
C
SCALE OF FEET
0 1 2 3 4 5 6
SCALE OF METRES
0 1 2

two pipes, thus allowing any kind of valve to be used for an upper valve. To the right-hand arm of the nozzle is attached a wrought-iron return-pipe, the open lower end of which dips into the tank O. This tank O is really no part of the pump, but a part of the reservoir R, and is in open communication with it by means of the pipe P, the object of the arrangement being to discharge the water from the lower pipe without the loss of head which would occur if the water were allowed to pour out of the nozzle and fall freely into the reservoir R. Across the nozzle at X and separating the lower and right-hand portion of the same from the upper and left-hand part, is a flange X, which may be either cast in the nozzle or made separately and bolted to its place.

The lowest pump is like that described in Fig. 1, except that the valve is a circular disk valve.

The following are some of the principal dimensions:

No. of pumps, ...	4
Height of each lift, ...	250 ft.
Size of each pump, ...	17 "
Interior diameter of pipe, ...	12 "
Thickness of pipe, lowest $\frac{3}{16}$; 2d, $\frac{3}{16}$; 3d, $\frac{5}{16}$; 4th, $\frac{7}{16}$ inches.	
Weight of water raised on each half of the stroke, approximately, ...	50,000 lbs.
Total weight of moving parts (pitwork), . . .	59,460 "
" " counterbalance required, . . .	59,460 "
Total mass of pitwork and water set in motion at both parts of stroke, ...	168,920 "
Or, ...	84.46 tons.

Some of the corresponding details for an 18-inch pump of the ordinary Cornish type (which would be of equal capacity), are:

Weight of rod and attachments, ...	114,576 lbs.
" " 18-in column water (1000 feet high), .	110,000 "
Counterbalance required, ...	59,576 "
Total mass, pitwork and water, set in motion on down stroke, ...	284,152 "
Or, ...	142.076 tons.

The mass set in motion on the upstroke would be the same less the weight of the column of water.

While in such a plant as that illustrated in Figs. 5 and 6 there is great saving in the weight of moving parts, and a consequent reduction in the mass set in motion, yet the chief reduction in

cost of construction would lie in the decreased weight of the fixed parts. Attention has been called to this in connection with Fig. 1.

Fig. 7 shows a second form of water-balance pump, in which the auxiliary chamber C, instead of being a part of the pump itself is a separate structure connected with the pipe by means of the elbow L and nozzle N. In this form of pump the main strain is carried in a direct line to the pump below by the pipe P, while the pump is at one side and the water-balance chamber at the other, the discharge of the water from the pump below taking place through the ports p p, as shown into the open-topped tank or sleeve, which is in direct communication with a tank or reservoir not shown in the drawing.

It is obvious that by disconnecting the joint I H and covering the opening with a flange, the pump becomes a moving-pipe pump similar to that described in Fig. 6, but with a different method of discharge for the water from the pump below; and further, that access to the valves, the removal of the plunger, either of the pump, or water-balance chamber, may be effected with the greatest ease; and also that the base being cast in one piece, the whole structure may be easily kept in line by means of the keys shown.

Fig. 7 shows another arrangement of pump barrel, water-chamber and pipe, by which less room is required in the shaft.

The pump shown in Fig. 7 is supposed to be one of a series 250 feet apart, and so situated that an upward pressure of 5450 pounds from the water-balance is required during the up stroke. This is accomplished by making the water-balance plunger 50 inches greater area than the pipe, thus giving as aid in raising the pipe the pressure induced by a column of water 50 square inches in base and 250 feet high, equal to 5454 pounds.

Fig. 8 shows, in vertical section and plan, a water-balance pump, in which both valves are stationary. As will be seen by an inspection of the drawing, the arrangement of the valve-box O, with the upper valve *v*; the H-piece T with the lower valve *u*; the plunger barrel L, with its plunger P may be the same as with an ordinary Cornish plunger-pump, as shown at k, in Fig. 1.

On the upper valve-box O, is a plunger-barrel D, with a stuffing-box *a*, at its upper end, through which works a hollow plunger E, quite open at its lower end, and at its upper end communicating with the upper portion of the moving pipe I, by means of the cast iron or steel nozzle n, n'. The plunger P, is connected with the main pipe I, by means of the cross-head $x\ x'$. The main pipe I, which conveys the strain in a straight line to the lower pumps, is

FIG. 6.

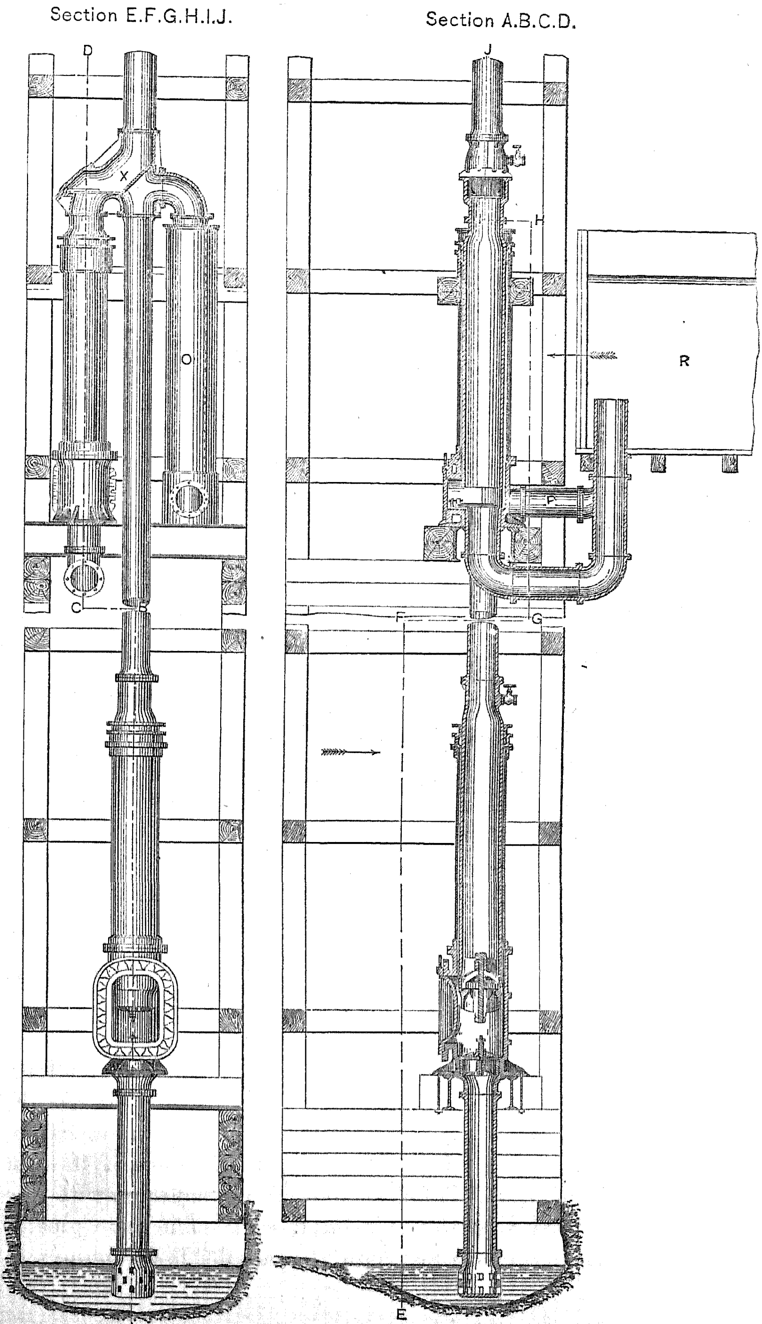


FIG. 7.

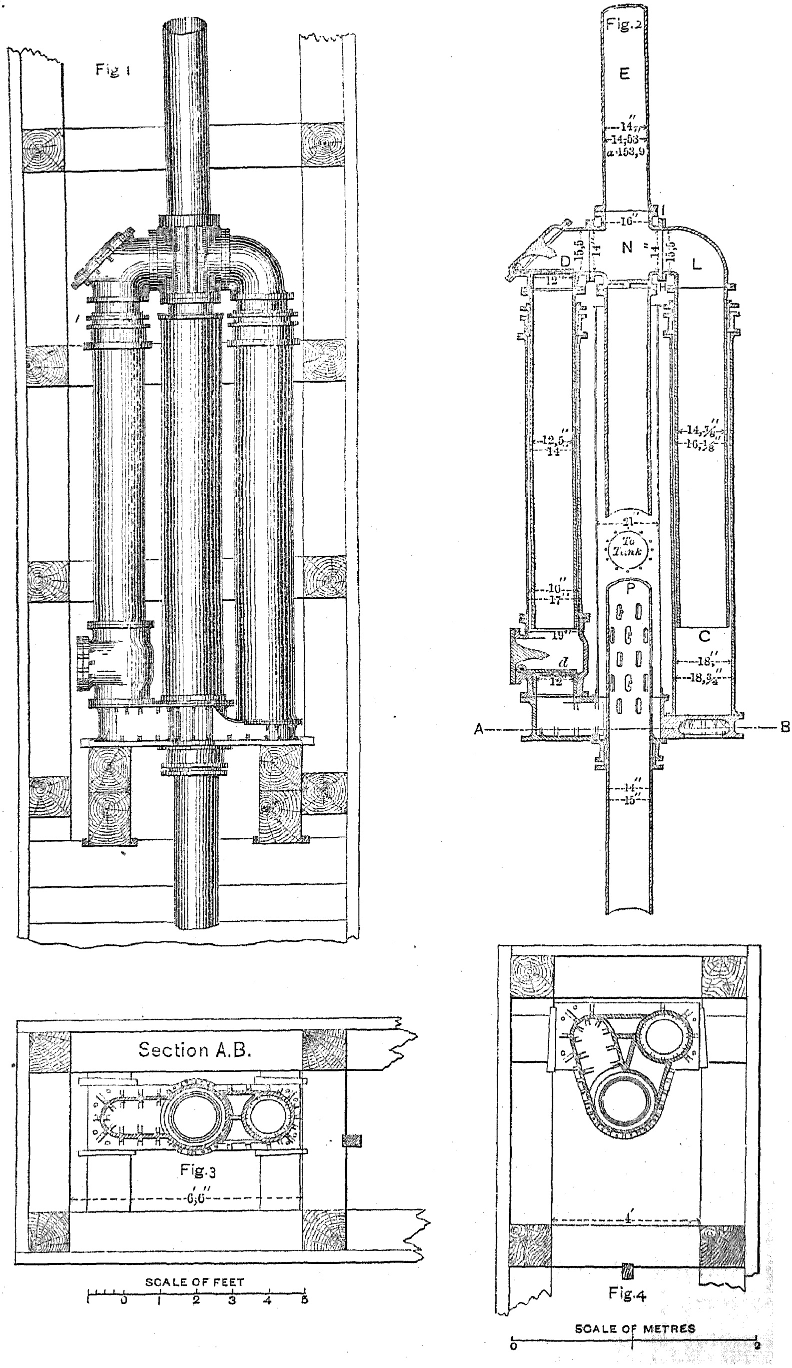


Fig.1
Fig.2
E
N
D
L
H
P
C
d
A
B
To Tank
Section A.B.
Fig.3
Fig.4
SCALE OF FEET
0 1 2 3 4 5
SCALE OF METRES
0 1 2

represented in the vertical section, as passing behind the plunger barrel D, and the valve-box O, and through a discharge-tank A, similar to that shown at M, in Fig. 1, or in Fig. 5. The discharge-tank A, and the suction-pipe S, connect with the reservoir R R′, by means of the pipes B B′, and C C′.

It is obvious, with this form of pump, that when the exterior diameter of the plunger E, is the same as the interior diameter of the pipe I, there will be no water lifted on the up stroke; the entire column of water remaining stationary, supported by the valve v, while the pipe moves up around it; the action in this respect being analogous to the Cornish plunger-pump. If now the exterior diameter of the plunger E be made greater than the interior diameter of the pipe I, then, on the up stroke, the column of water will descend, occupying the enlarged space created by the withdrawal of the plunger E, and, by just so much as it descends, assist in raising the pipe and its attachments.

Any required degree of counterbalance may therefore be obtained, by a regulation of the area of the plunger E. As with other forms of water-balance pump, the capacity of the pump per stroke is measured by the area of the plunger P.

In the form of pump shown in Fig. 8, there is required the same valve-box and H-piece, or pieces corresponding to them, that are found with the Cornish pump; hence in weight of fixed parts of the pump itself there is no economy. Its advantage appears to be mainly in the use of a single line of lap-welded tubing, as pump-rod, mounting pipe, and water-balance pipe, and in the ease with which the principle may be applied to the ordinary pumps now in use.

In looking over the forms of the two pumps here proposed, it will be noticed that the subject naturally divides itself into two parts: A, a simple and practicable method of causing the water to enter and ascend in the pipe; and B, a simple and practicable method of getting the water out of the pipe at its top.

A. The water is caused to enter and ascend the pipe mainly by the valves, and of these there are four arrangements. 1st. That shown in Fig. 1 and in the bottom of Fig. 6, in which the lower valve is stationary in an ordinary "door-piece" or valve-box, and the upper movable valve is attached to the bottom of the plunger by the single central bolt, both of these valves being accessible through the same valve-chamber. In this case the upper valve does not admit of the commonly used flat Cornish valve, but requires a

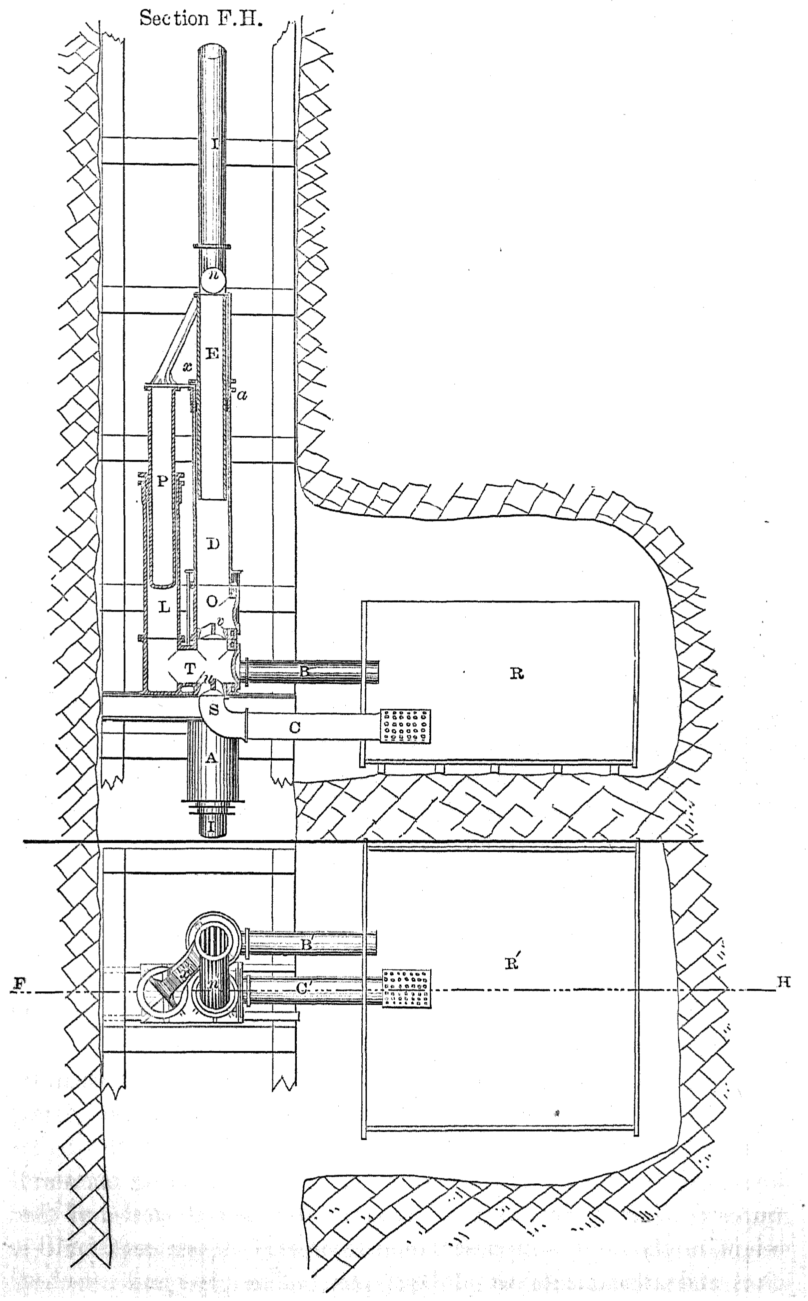
disc valve, as shown in Fig. 6, a six-sided valve, as shown in Fig. 1, or some valve of like construction. 2d. The form shown in Fig. 8 and in the upper part of Fig. 6, in which both movable and stationary valves are so situated in a valve-chamber that any kind of valve may be employed. With this arrangement the upper valve is as accessible as the lower valve and admits of as great port area, advantages not possessed by the first arrangement. 3d. In which the upper valve is in a piston or bucket, as in Fig. 3. 4th. In which the valves are arranged as in the ordinary Cornish pump, see Fig. 8.

B. There are shown five methods of discharging the pipe at the top. 1st. As at M, Fig. 1, and as in Figs. 6 and 7, in which the pipe passes through an inverted stuffing-box, allowing the water to escape into the surrounding sleeve or tank through the ports in the pipe, the aggregate area of which should exceed the area of the pipe. It is, of course, understood that the pipe should be made of unusual thickness to allow for the port openings and the wear incident to the stuffing-box. 2d. As at O, Fig. 1, where the pipe passes through an inverted stuffing-box and ends within the tank. This arrangement can only be used when the strain is taken by side pipes or timbers, as in Fig. 1 or in Fig. 4. 3d. As at N in Fig. 1, where on the top of the moving pipe, and in direct connection with it, is mounted a tank into which dips the suction-pipe of the next pump above. (This tank must be connected by means of a 3″ or 4″ hose with a large reservoir at the same level.) This arrangement, which possesses the advantage of being without the friction of stuffing-boxes, would be practicable only with short-stroke pumps, cold water, and at a low altitude, as the water for the pump above would have to be drawn by suction to a height at least twice the length of the stroke. 4th. As at Fig. 4, where the slightly enlarged pipe is provided with a stuffing-box through which works the suction-pipe of the upper pump, which suction-pipe is connected with the reservoir as shown, thus deluging the lower valve of the upper pump if desirable. 5th. As at Fig. 7, in which the water passes through the nozzle and out through the return tube into the tank O.

It is also quite easy to allow the water to pour out of the nozzle into a tank, thus avoiding the return tube; but as such a method would involve a loss of head equal to nearly or quite the length of the stroke, it is not shown.

Of some fifteen forms of water-balance pump possible, three are

FIG. 8.



here shown as most simple and practicable. As nearly any of the forms of pump shown may be combined with any of the different methods of discharge, and as either the moving-pipe pump or the water-balance pump allow the employment of almost any sort of valve, it is evident that a great variety of constructions to suit varying conditions is admissible.

Explanation of Table.—In order to institute a comparison between the ordinary Cornish system with wooden pump-rod, and the moving-pipe pump and water-balance pump, I have calculated the following table, in which the principal weights of the three systems as applied in several series of 14-inch pumps, from 1000 to 4000 feet, total depth, admit of comparison.

The factor of safety for tensile and compressive strain, of both rod and pipe, has been, in all cases, taken at 8. Both rods and pipes are supposed to be accurately guided every 40 feet. In proportioning the tapered wooden rod, the following data are assumed. The specific gravity of wet pine is taken at .75, its ultimate tensile strength at 10,000 pounds per square inch. The strapping plates and bolts are supposed to increase the weight of the rod by 80 per cent., and the joints to decrease its tensile strength by 25 per cent. The branch rods, plungers, and other attachments are supposed to increase the total weight of the main rod by 30 per cent. The ultimate thrust is found from C. S. Smith's formula, in which the breaking load of pine in short blocks is assumed as 5000 pounds.

Each pump, except the three lowest, is supposed to be provided with its own underground balance-bob. The pumps are 14 inches diameter, with 8-foot stroke, and are situated 250 feet apart. The pump-rod is tapered from 17.3 inches square, at the top of the 4800 foot lift, to 8 inches square at the bottom.

In proportioning the pipe for the moving-pipe pump, which is supposed to be of the type shown in Fig. 6 or Fig. 7, with the joint I H covered with a flange, the interior diameter is taken at $11\frac{1}{8}$ inches; giving an area of 96.7 square inches.

The ultimate tensile strength of the iron employed is taken at 48,000 pounds per square inch, and the ultimate compressive strength, in short blocks, 36,000 pounds. The compressive strength of the pipe is calculated by Gordon's formula. All the moving parts are supposed to be made either of wrought iron or cast steel, and the weight of the pipe is supposed to be increased 50 per cent. by the joints and attachments, as plunger, etc. The pipe and attached

moving parts are supposed to be *entirely* counterbalanced by means of bobs situated at every pump, or 250 feet apart.

The water-balance pump, referred to in the table, is of the type shown in Fig. 7, this plate being the drawing of the eighth pump from the bottom, or the uppermost pump of the 2000-foot lift, and the middle pump of the 4000-foot lift. With this pump, also, all moving parts are supposed to be made of wrought iron or cast steel. The area of the water-balance plungers increases from 132.7 square inches in the bottom pump, to 323.6 square inches in the sixteenth pump from the bottom, or 3750 feet from the bottom. The area of the interior of the pipe is the same as the pump-plunger, viz., 153.94 or 14 inches diameter.

Is is hardly necessary to say that the water raised during the up-stroke (for in the four lowest pumps of the series the water-balance plunger, being smaller than the pipe, some water is lifted), as also that which descending acts as counterbalance, is included in "mass set in motion" of the water-balance pump.

The column "mass set in motion" is, in all cases, the greatest mass in motion, whether on the up-stroke or down-stroke.

No allowance whatever has been made for friction in calculating the dimensions for the table.

In making this comparison it would be obviously unfair to the wooden-rod system, to compare an accurately tapered pipe with a wooden rod of the uniform size from top to bottom. As in the Comstock practice, where the wooden-rod system has been carried to its greatest depth, no use, or very little use, is made of tapered rods, I have been obliged to suppose a case, in which all possible weight has been saved by tapering the rod, so that it may have a maximum of strength with a minimum of weight, and to neglect actual data from the Comstock in my possession. The actual weight of moving parts and of "mass set in motion," of any plant on the wooden-rod system now in operation on the Comstock lode, would be, for a 14-inch pump, largely in excess of the weights given in the table. So that a comparison of either of the two new systems with the wooden-rod system, as *actually applied* on the Comstock, would show a "mass set in motion" considerably less than half that required in actual Comstock practice.

It is also true that the theoretical weights of the moving pipes would, in practice, be somewhat increased, owing to the fact that too frequent change in size and weight of the variable parts would lead to too great a complication in construction and repairs.

Table of Weights of the Wooden-rod System compared with Moving-pipe and Water-balance Systems, for a Series of 14-inch Pumps, at various depths:

		Weight of column of water.	Tensile strain at top of rod or pipe.	Thrust at top of rod or pipe.	Total weight of rod or pipe with connections.	Total weight of underground counter-balance needed	Weight of equalizing counterbalance in surface-bob	Weight of total mass set in motion.	Size of square wooden rod.	Metal-area of top pipe in square inches.
Total depth,	Wooden rod..	66,496	76,520	10,024	58,170	3,326	43,274	189,616	9 2	....
1000 feet,	Moving pipe	"	41,800	24,692	36,095	36,095	8,554	122,545	....	8 8
4 pumps.	Water balance	"	49,942	16,552	42,082	.... ..	16,695	117,418	....	8.4
Total depth,	Wooden rod..	99,744	104,188	4,444	106,470	23,958	54,316	306,164	11 2	... .
1500 feet,	Moving pipe	"	62,700	37,038	66,657	66,657	12,831	208,845	....	13.2
6 pumps	Water balance	"	74,910	24,834	70,911	... ...	25,038	190,690	....	12.5
Total depth,	Wooden rod. .	132,992	130,616	2,376	170,520	61,580	64,120	450,888	12.7	....
2000 feet,	Moving pipe .	"	83,610	49 384	108,343	108,343	17,108	317,404	....	17.6
8 pumps.	Water balance.	"	99,978	33,117	110,224	....	33,380	280,880	....	16.6
Total depth,	Wooden rod	166,240	157,014	9,136	249,690	114,262	74,939	626,807	14 1	....
2500 feet,	Moving pipe. .	"	104,500	61,730	161,154	161,154	21,385	448,193	....	22 9
10 pumps.	Water balance.	"	124,846	41,400	160,021	....	41,722	388,003	....	20.8
Total depth,	Wooden rod.. .	199,488	182,512	16,976	343,770	182,934	82,786	830,654	15.2	....
3000 feet,	Moving pipe. .	"	125,400	74,076	225,088	225,088	25,662	601,238	....	26 5
12 pumps	Water balance	"	149,814	49,683	220,302	.... ..	50,064	512,041	....	24.9
Total depth,	Wooden rod ...	232,736	207,080	25,656	452,550	267,146	90,712	1,054,820	16 3	....
3500 feet,	Moving pipe .	"	146,300	86,422	300,146	300,146	29,939	776,531	....	30 8
14 pumps	Water balance.	"	174,782	57,966	291,067	. ...	58,406	52,999	....	29.1
Total depth,	Wooden rod . .	265,984	231,928	34,056	575,400	365,148	98,936	1,327,144	17 3	....
4000 feet,	Moving pipe. ..	"	167,200	98,768	386,328	386,328	34,216	974,072	....	35 3
16 pumps.	Water balance.	"	199,750	66,248	372,316	....	66,748	810,879	....	33.2

For convenience in calculating the weights given in the table, the weights of the parts attached to the rod or pipe, and moving with it, such as the plunger with its connection, the branch rods, etc., have been for each system assumed to constitute a fixed proportion of the weight of the rod or pipe for each pump; the proportion taken being that of the middle pump of the series. The total weights, therefore, given in the table, will be for depths less than 2000 feet, somewhat too small, and for depths greater than 2000 feet, too great. As the plan has been followed in the three systems, the comparative value of the table is not affected.

A careful consideration of this paper will show that while in simplicity of construction, accessibility of valves, and in general ease of construction and repair, the systems here proposed are in no way inferior to the Cornish system, they possess unquestionable advantages over that system in the following respects:

1st. As in either of the two systems proposed, the moving pipe replaces both the rod and mounting pipe of the old system, the first cost of the plant is very considerably lessened.

2d. Either of the systems requires far less room in the shaft, and consequently less cost for excavation and timbering, than the wooden rod system.*

3d. All the moving parts subject to great tensile strain being hollow, the entire structure admits of an inexpensive hydraulic test in the shop, thus reducing the chance of breakage to a minimum.

4th. Inasmuch as the depth to which the present system can be carried is limited by the magnitude of the "mass to be set in motion" at each stroke, and as in the new system this mass is very materially reduced, it follows that a much greater depth can be attained with the new than with the old system.

5th. That for any depth, the mass to be set in motion being much less, the velocity of the mass may be greater, and that, therefore, either a smaller pump may be used, or with the same size of pump a greater useful effect may be had.

The increase of useful effect due to this decreased mass set in motion, I estimate from one-half to two-thirds the entire useful effect of the wooden-rod system.

SALT LAKE CITY, Nov. 11, 1878.

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* This is especially true of the water-balance pump, which avoids entirely the use of underground balance-bobs. The excavation and timbering of the chamber, the construction, erection, and attachment of the underground bobs in common use on the Comstock lode to-day, cost from $2500 to $4000 for each bob, and the common practice is to have one for every two pumps, or say 400 feet apart.

A DIRECT PROCESS OF COPPER SMELTING.

BY H. M. HOWE, A.M., E.M., BOSTON, MASS.

(Read at the Lake George Meeting, October, 1878.)

MANY direct processes have been proposed for the treatment of oxidized ores of copper by reducing the copper oxide to the metallic state, and by separating it from its impurities by a subsequent fusion. The radical objection common to all the methods which have come to my notice, except those in which sulphur plays the prominent part, is that the very deoxidizing agent which effects the reduction of the copper oxide, inevitably reduces at the same time a very considerable part, if not the whole, of the iron which almost invariably accompanies copper in its ores. The result is that, when the ore is subsequently fused, a very great quantity of the already reduced iron separates from the slag along with the copper, alloying with it, and necessitating costly subsequent operations for its separation. Thus, at Perm, in the Urals,* the product of the fusion of ferruginous copper oxides is a mixture of one hundred and fifty-five parts of highly cupriferous pig iron with two hundred and five parts of very impure and ferruginous black copper, containing some 10 per cent. of carbon and iron, though free from sulphur, and of course very difficult to refine.

If, in order to avoid this reduction of iron to the metallic state, the reducing action be made feebler, it inevitably results that a considerable part of the copper as well escapes deoxidation; and, consequently, the slags are so rich that it becomes necessary to treat them subsequently in order to recover the copper which they contain. Thus, in treating native copper, at Lake Superior, where the reducing action is purposely made feeble in order to prevent the deoxidation of the iron, the slags from the first fusion of the ore carry so much copper as to render it imperative to treat them again, which is done in a cupola. And yet, in spite of so far weakening the reducing action as to cause a notable slagging of the copper, a very considerable reduction of iron actually takes place.

These are the two dangers which threaten the smelter of oxidized copper ores. If he wishes to avoid slagging his copper, he must have so strong a deoxidizing action as to reduce a great deal of his

* Rivot, Principes Généraux du Traitement des Minerais Métalliques, i, p. 79.

iron to the metallic state, and so produce a very ferruginous black copper, very difficult to refine; and, if he would steer clear of Scylla on this side, he inevitably strikes Charybdis on the other; for, if, in order to lessen the amount of iron in his product, he enfeebles his reducing action, it will not be strong enough to prevent a great slagging of his copper.

Apparently the only way to avoid these two dangers, which threaten on either hand, is to employ a reducing agent which is capable of completely deoxidizing copper oxide, but which is unable so fully to deoxidize iron oxide that, on subsequent fusion, any of this metal should separate in the metallic state.

While studying this subject with our fellow-member, Mr. W. E. C. Eustis, a mixture of carbonic oxide, or other strongly reducing gas, with some less strongly reducing, or even with some non-reducing, gas, such as carbonic acid or vapor of water, suggested itself as an agent fitted for this work. It was believed that, owing to the fact that copper has a much weaker affinity for oxygen than iron has, it might be possible to mix two or more of such gases in such proportions that, while the mixture should be able to entirely rob the copper of its oxygen, it should only be able to remove a slight portion of the oxygen which was combined with the iron.

It was further believed that, if the ore were melted after this treatment, without having been exposed to any conditions which would reoxidize the copper, this metal, being wholly in the metallic state, would completely separate out from the slag, while the iron, still retaining a considerable quantity of oxygen, would remain wholly in the slag.

The following experiments which we tried threw considerable light on the subject:

Experiment 1.—Some CuO, previously ignited in O, was exposed in a glass combustion-tube to a stream of CO, which had been passed through caustic potash, and then through chloride of calcium. At the ordinary temperature the gas escaping from the combustion-tube did not produce the faintest turbidity in lime-water. The temperature was very gradually raised. It was not until the CuO had reached a temperature of from 85° to 90° C. that the least turbidity arose, but on reaching this temperature the lime-water became distinctly turbid.

This experiment clearly showed that CO attacks CuO at a temperature much below that at which it first begins to act on Fe_2O_3;

since Bell's careful experiments* have shown that CO does not reduce the most porous forms of Fe_2O_3 below a temperature of 141° C., while the denser forms of Fe_2O_3 refuse to yield up any of their O below a temperature of 200° C.

EXPERIMENT 2.—To discover the rate at which CuO becomes reduced by CO at these low temperatures a lot of the same CuO was exposed at a temperature of 200°C. to a stream of CO. At the end of four hours the CuO had lost about 10 per cent. of its O.

EXPERIMENT 3.—A lot of similar CuO was exposed to a stream of CO for one hour at a temperature of 325° C. At the end of this time 97 per cent. of the initial oxygen had been removed.

By referring to Bell's experiments, 19 to 48 (*loc. cit.*), it will be seen that copper oxide, even when in this compact form, is reduced at these low temperatures much more rapidly than the most porous and easily reducible forms of iron oxide.

These experiments having indicated that copper oxide is acted on by CO at much lower temperatures, and much more rapidly than iron oxide is, we next proceeded to study the effect of a mixture of equal volumes of CO and CO_2 on CuO, similar to that previously used.

EXPERIMENT 5.—Pure CuO was exposed for twenty-five minutes to a stream of equal volumes of CO and CO_2 at the temperature of melting zinc. 1.1 per cent. of O remained.

This reduction is much more rapid than that performed by Bell (*loc. cit.*, Experiment 379). This may be due to the stream of gas being more rapid, or to the CuO being in a more porous condition in our experiment.

EXPERIMENT 6.—Pure CuO was exposed to a stream of equal volumes of CO and CO_2, for thirty minutes, at a dull-red heat. The product was highly sectile and metallic, the reduction being apparently nearly complete.

EXPERIMENT 7.—Some native malachite was exposed to the same gas for thirty minutes at a dull-red heat. The residue was thoroughly digested in dilute H_2SO_4: in the solution were found only 1.25 per cent. Cu. Supposing this to have existed solely as CuO, 1.25 per cent. only of Cu had not reached the metallic state. Even if it had all existed as Cu_2O, all but 2.5 per cent. of the Cu must have been in the metallic state.

* Journal of the Iron and Steel Institute, 1871, vol. i, p. 98.

EXPERIMENT 8.—Treated some very compact native chrysocolla, in the same way as in Experiment 7. 3.6 per cent. of copper was soluble in dilute H_2SO_4, indicating that between 3.6 and 7.2 per cent. of copper remained combined with oxygen. In the residue we found 10.8 per cent. copper, soluble in boiling aqua regia, which would indicate that between 10.8 and 7.2 per cent. copper has passed to the metallic state.

This experiment shows what we would naturally have expected, that compact chrysocolla is attacked much more slowly than the porous malachite, whose natural porosity is increased by the expulsion of its CO_2. Still, that the copper in so strong a chemical compound as copper silicate, shou'd have been robbed of nearly half its oxygen by so weak a reducing agent as was here employed, and in so short a time, was certainly most encouraging.

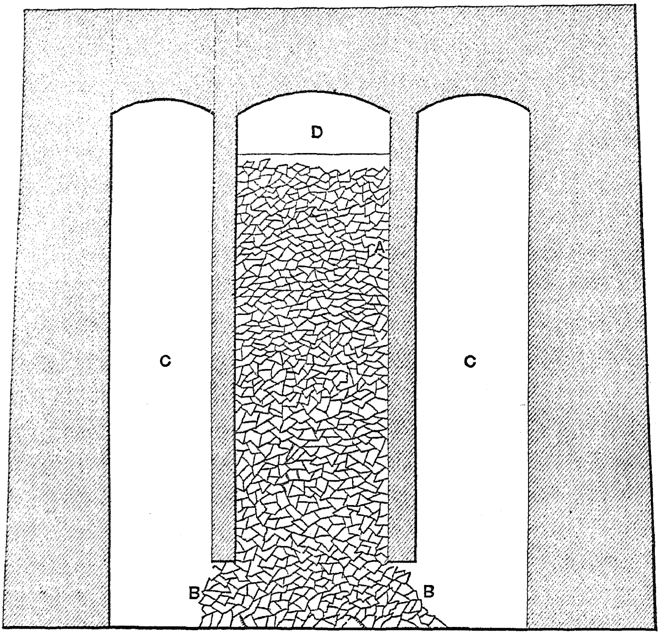
The experiments just quoted are fully confirmed by Bell's (*loc. cit.*, p. 174), which showed that equal volumes of CO and CO_2 not only were unable to oxidize spongy Cu, but completely reduced CuO to the metallic state at a red heat.

This same mixture of gases had already been demonstrated by Bell to be incapable of reducing iron oxides to the metallic state at a red heat; indeed its action is to *oxidize* metallic iron to the state of FeO, to which state it reduces higher oxides of iron as well (*loc cit.*, p. 112). Here then we have an agent which is capable of rapidly and completely reducing copper oxide to the metallic state, but is incapable of so far reducing iron oxide that, on subsequent fusion, any of the latter metal will separate in the metallic state. The evidence afforded by the experiments I have just quoted seemed so very encouraging, that it was decided to try on a commercial scale, a process based on the principles already set forth. This was done at the smelting works of Messrs. A. Hemenway & Co., the Hornos del Norte, at Caldera, Chili, under my personal supervision.

The process, as carried out on a large scale, naturally divides itself into two parts, (1) the reduction, and (2) the subsequent fusion—the scorification of the iron and the separation of the copper in the metallic state.

I. *The Reduction.*—The apparatus employed for this is shown in transverse section in the accompanying sketch. It consisted of a central rectangular retort, *A*, in which the ore was placed, and two rectangular chambers, *C C*, on either side of the retort, in which gas was burned for the purpose of heating the ore in the retort,

to the temperature of rapid deoxidation. The gas used for the reduction was forced in at *D* at the top of the retort, found its way downwards through the column of ore, and escaped from the bottom



of the retort, through the apertures, *B B,* into the combustion-chambers, *C C.* In the latter any CO which had not been oxidized to CO_2 in passing through the retort, *A*, was burned. In addition to this a further supply of gas was admitted into the chambers, *C C*, direct from the gasogènes.

As soon as the copper oxide had been thoroughly reduced the supply of gas was stopped, both in the retort, *A*, and in the chambers, *C C,* and the charge was either dropped at once through the bottom of the retort into the melting furnace, or was allowed to cool in a non-oxidizing atmosphere. For regular working, a continuously working furnace, with a cooling apparatus at the bottom, like Blair's sponge furnace, would have been preferable.

The gas injected into the column of ore contained from 6 to 13 per cent. of CO_2. The means of supplying the CO_2 was very simple. It consisted simply in keeping a thin layer of fuel in the gasogène,

and in stirring it only at rare intervals; in short, it was hardly necessary to do more than to direct the workman to neglect his fire. At first it was necessary to make frequent assays of the gas that was designed for the work of reduction; but, after a little experience, the workmen were able to judge by the eye when the gas was of the right composition. By keeping the composition of the gas within these limits, it was found that the copper was thoroughly and rapidly brought to the metallic state, while no evidence was given of the iron being considerably deoxidized. At a temperature approaching dull redness it was found that the operation went on with rapidity. Four hours' exposure sufficed to completely reduce the ore which lay in the upper part of the retort, and which was consequently exposed to the gas before the reducing power of the latter had been at all expended. Of course the time needed for reducing the whole column depended upon the rapidity of the stream of gas, for but little reducing power could be exerted upon any particular layer of ore until the copper in the layers above it had been considerably deoxidized. This is easily understood when we consider how weak a hold copper oxide has upon its oxygen; so weak is it, that one of the easiest methods of completely converting CO into CO_2, is to pass it through copper oxide. From this it would be inferred that the reducing power of the CO and H which were contained in the gas, would be very fully utilized during its passage through the ore; and this was the case, the percentage of CO_2 in the gas in the lower part of the retort being occasionally as high as 54, the remainder being principally N. When the deoxidation of the copper oxide was nearly complete the proportion of CO_2 in the escaping gases of course fell; and this afforded a very clear and easily recognized indication that the process was nearly complete. For as long as nearly the whole of the CO of the gas was being converted into CO_2 by the ore, and was thus rendered non-combustible, it would not burn on issuing from the retort, *A*, into the chambers, *C C*; but when the reduction of the copper oxide was nearly complete, the CO of the gas would be but partially oxidized into CO_2, and, on issuing into the chambers, *C C*, the unoxidized CO would give the workman notice of its presence by the bright flame which issued from the apertures, *B B*.

I would take this opportunity to testify to the great convenience and efficiency of the Orsat apparatus for the analysis of furnace gases. In these and the other experiments which I made at Caldera,

I made somewhere in the neighborhood of one thousand gas analyses with one of these excellent instruments; I found it of the very greatest use in interpreting the somewhat complicated phenomena, which, without it, would have been very difficult to understand.

It was found that reduction went on tolerably fast at very low temperatures. Indeed, the gas escaping from the lower part of the retort showed a considerable proportion of CO_2 when the ore was still so cool that one could easily bear his hand for a few seconds on the cast-iron walls of the retort. With increasing temperatures the rapidity of the deoxidation was greatly increased; and at the highest temperature which was used, which was a dark-red heat, the reduction went on much faster than at any lower heat.

The product of the exposure of copper oxides to this operation was of a bright copper-color, and of a spongy texture (indeed, analogy would lead us to call it copper *sponge*). It could be easily broken in the fingers, although the original ore was, in many cases, exceedingly compact and hard. The gangue naturally was in general entirely unaffected; some lumps of pyrite, of chalcopyrite, and of other copper sulphides, which were experimented upon, were but slightly affected.

If the exposure were kept up for a great length of time, the lumps of sponge were frequently found to be coated with a black carbonaceous matter, which may have arisen from the splitting up of CO, or may have merely been a deposition of the soot, which is copiously developed by the Chilian coal. It was found that the sponge rapidly absorbed oxygen from the air at comparatively low temperatures. Although no accurate observation of the temperature at which oxidation commences was made, my observations incline me to think it could not have been far above 212° F. (100° C.)

On some occasions when the sponge was exposed to the air before being thoroughly cooled, the absorption of oxygen was so rapid as to cause an intense elevation of temperature. Thus, one charge was drawn when the greater part of it was stone cold; a small part of it was, however, slightly warm. This oxidized with such rapidity that the rest of the charge was soon raised to the temperature of oxidation, and, although a copious quantity of water was thrown on it as soon as it was perceived to be burning, yet almost the whole of the pile became oxidized before we could check the combustion. While thus rapidly oxidizing, a beautiful play of colors was to be seen on the exterior of each lump of ore.

When kept quite dry, the sponge seemed to have little tendency to oxidize; but if it was exposed to the night air, which is exceedingly damp at Caldera, it gradually became carbonated, and was converted into a green and very friable mass. If, however, it were but slightly protected, as by being placed in bins, or in sheds, it would remain unaltered for considerable lengths of time.

Although the ore was never raised above a dull-red heat in these operations, and was consequently always far below the temperature of incipient plasticity, a very rapid formation of moss copper took place. Many of the lumps of sponge (for the size and shape of the lumps of ore were not materially changed by this reducing operation, coming out as they went in, of about the volume of a two-inch cube) were covered with a coating of fine curly hairs of moss copper, the individual hairs sometimes reaching a length of two inches during the short period when they were in the reducing retort. I am aware that a slow growth of moss copper and silver has often been observed in cabinets, even at the ordinary temperature; but I was not at all prepared for this extremely rapid growth, at so low a temperature as existed in this reducing furnace.

Melting the Sponge.—Since the copper smelting furnaces at Caldera, which are of the ordinary Welsh reverberatory pattern, can be so worked as to have a very mildly oxidizing atmosphere, it was at first hoped that it might be possible to drop the sponge from the reducing retort directly into the reverberatory (over which the retort was built with this design), without waiting for the tedious operation of cooling it down. But these furnaces have a very flat bottom, so as to expose as great a surface of the ore as is possible to the flame, to hasten the fusion. This, of course, prevented our covering the sponge with slag, or in any way protecting it from the direct action of the flames; the consequence was that the sponge lay entirely naked, until it was completely melted, and, although the atmosphere was kept as mildly oxidizing as possible, yet the exposure was so long that a considerable slagging of the copper took place.

To avoid this oxidizing exposure, a very deep basin-like furnace was built, so that a small quantity of slag could cover a great quantity of melted copper, or of unmelted sponge, and protect it from oxidation. Before charging any sponge in this, a small charge of shells and slag was melted in the basin, so as to form a bath with which the sponge could be covered. When this was quite fluid the

sponge was charged in small quantities at a time, the atmosphere being kept as non-oxidizing as possible.

The sponge was very light, and floated on the slag as corks do on water. As the heat of the furnace was very high, and as the slag was correspondingly liquid, it was found easy to protect the sponge from the direct action of the flames by stirring it vigorously with a rabble, and mixing it with the slag. A minute's stirring after charging each lot of sponge was sufficient to cover it thoroughly with slag. To further guard against oxidizing the sponge before it was well protected with the slag, the atmosphere of the melting furnace was overcharged with gas, the air-valve being almost entirely closed. The percentage of CO_2 in the middle of the reverberatory was from 11 to 13, while, when the atmosphere of the furnace was clear, it contained only from 5 to 8 per cent. of CO_2. At the same time the draught was checked, to prevent the entrance of any air from the working door.

In some cases a quite violent ebullition took place immediately after charging the sponge, lasting, however, only a few minutes, after which the surface of the bath was perfectly tranquil. This ebullition may have been due to moisture adhering to the sponge, or to the sudden expulsion of CO_2 from the calcite gangue which some of the ores treated carried, or to the oxidation of the carbonaceous matter which in some cases coated the sponge. I am inclined to attribute it to the latter cause, at least in some cases, since it was several times accompanied by the escape of minute blue flames, which burst through the slag very much as in a puddling or a black ash furnace. About twenty minutes was required to thoroughly melt each charge of sponge.

A slight quantity of very rich regulus was frequently found to accompany the copper which resulted from these operations. This probably arose in part from the presence of a small amount of sulphides in the ore, though none were visible; but principally from the fact that the furnace bottom was slightly impregnated with copper regulus, which it had absorbed from former operations in which sulphides had been used.

When this furnace was commenced, it had already been decided to close the smelting works, the price of copper having fallen so low that there was no margin for profit, and the proprietors not looking for any early permanent improvement in the market; this sponge-melting furnace was therefore built simply in order to test our pro-

cess, and was made no larger than seemed necessary to afford a satisfactory trial, its total capacity being only about 15 cwt.

Owing to the fine state of division of the sponge, some of it remained mechanically suspended in the slag even when the fusion was complete. It was therefore found expedient to allow it to remain tranquil for some time after it had been thoroughly melted, to allow the suspended copper to settle. Advantage was taken of this opportunity to employ the method of Rivot and Phillips of removing from the slag any copper which was chemically combined with it, by inserting iron rails into it. When the iron rail had been brought up to a red heat, it was inserted into the upper part of the layer of slag, and gently moved about, so as to bring it into contact with fresh portions of slag, care being taken not to allow the rail to dip so deep as to come in contact with the bath of copper which lay underneath in the bottom of the basin. By observing this precaution, the danger of the iron melting and alloying with the copper is entirely avoided. The copper chemically combined with the slag was rapidly precipitated on the rail, the latter becoming coated with a bright-yellow film of copper in the course of a few minutes. An hour generally sufficed to thoroughly impoverish the slag.

The action of the iron rails is so clear that I need hardly describe it. Since copper has a lower affinity for oxygen than iron has the oxide of copper in the slag yields up its oxygen to the iron of the rail, and passing into the metallic state releases the silica with which it was combined in the slag, which latter instead combines with the iron which has just become oxidized at the expense of the copper oxides; as in the reaction

$$CuO(SiO_2)_x + Fe = FeO(SiO_2)_x + Cu.$$

In this way, what with the mechanical settling out of suspended metallic copper, and with the deoxidation of the copper oxides of the slag by means of iron rails, the tenor of the slag was quickly reduced from 1.5 or 1.7 per cent. of copper to .7 or .8 per cent. The consumption of iron was very small, being as nearly as we could calculate about equal, pound for pound, to the copper which it deoxidized. A considerable portion of the .7 per cent. of copper which the slag contained was not chemically combined, as was clearly shown by the fact that, if the fresh fracture of the slag were rubbed, fine specks and streaks of metallic copper were made plainly visible. This was undoubtedly due, at least in part, to our having

to skim the slag from the surface of the metal, which agitated the latter more or less, causing it to mix to a small extent with the slag. Had the latter been allowed to flow off tranquilly through a large settling basin, or forehearth, I believe that the tenor of the slags might have been even lower than it was.

The metallic product was malleable, and contained from 97.6 to 98.5 per cent. copper, the former being the lowest limit reached in any of the heats made. When we consider the extremely low tenor of the slags, the high tenor of the copper seems most remarkable. As far as I am aware, there is no record of copper having been made in the dry way at a single operation, from ferruginous ores, with so slight a loss from slagging, and with a product so pure and so free from iron. This result proves the feasibility, even on a commercial scale, of our plan of thoroughly deoxidizing copper oxide with a mixture of gases which shall yet be unable to reduce iron oxide so far as to enable it to separate from the slag on melting, and to alloy with the metallic copper.

Conditions under which the Process is Applicable.—There are two sets of conditions under each of which it may be found advantageous to employ the process just described. The first is that sulphide ores are expensive; the second, that fuels fitted for cupolas (such as coke, anthracite, and charcoal, which we may call class A) are dearer than those unfitted for cupolas (such as bituminous coal, peat, wood, sawdust, lignites, etc., which we may call class B).

I. *Sulphides are Abundant.*—(*a.*) When there is an abundant supply of sulphides, and when fuels of class A are cheap, it will almost always be best to run oxidized ores down in cupolas, mixed with sulphides, since the consumption of fuel in this way will be much less than in our process; this will generally far more than counterbalance the advantages which our method offers of yielding a rich product.

(*b.*) If, however, fuels of class B be considerably cheaper than those of class A, cupolas should not be used in general. It will, in this case, be most advantageous either to use our process, or to melt the oxidized ores, together with sulphides enough to prevent slagging, in reverberatories.

It is rarely possible to obtain by this latter means a product carrying over 50 per cent. Cu without a serious loss of copper in the slag, while the expense of the operation is nearly as great as that which the use of our process entails. If freights are high, and fuel

for concentrating this 50 per cent. matte expensive, it will frequently be found more advantageous to obtain a rich product, at a single operation, by using our process than either to ship a 50 per cent. matte or to go to the expense of concentrating it. Thus at Caldera, where sulphides are abundant, fuels of class B dear, but still much cheaper than those of class A, and freights high, we found that the use of our process effected an economy of about $\frac{1}{2}$ cent per pound of copper treated over the ordinary Welsh reverberatory process, which is nearly universally used throughout Chili.

II. *Sulphide Ores Expensive.*—(*a.*) Under this condition, if fuels of class A are cheap, oxidized ores may be run down in cupolas. Though the consumption of fuel will be in this way less than with our process, the latter may still be the more advantageous. For, as already set forth, in running down oxidized ores alone in cupolas, the reducing action must be made so strong, in order to prevent slagging the copper, that a great quantity of iron will be at the same time reduced; while, with our process, we get a pure slag with a copper free from iron.

(*b.*) Finally, if sulphides are not to be had, and fuels of class A are considerably more costly than those of class B, our process will, in general, be found to offer very great advantages. For, in this case, cupolas of course cannot be used, and reverberatories must be resorted to; and in reverberatories it will, in general, be found much more economical to use fuel in the gaseous than in the solid state, especially with lignite, sawdust, peat, and such inferior fuels. Having gasogènes then for the reverberatory, the expense of previously treating the ore in our deoxidizing furnace will be found slight. Almost the only other alternative would be to deoxidize the ore on the hearth of the reverberatory, by either prolonged heating in contact with solid carbonaceous matter, or by inserting iron bars into the melted ore, or by combining both means. But the former method (heating in contact with solid carbonaceous matter) is inevitably attended either with a very great loss from slagging, or with the production of a very impure product, contaminated with iron reduced by the solid fuel, or more generally still with both these inconveniences; while the latter method necessitates a very great consumption of iron. A combination of the two methods (*i. e.*, a preliminary heating in contact with carbonaceous matter followed by a fusion, and by subsequent impoverishment of the slags by means of iron bars) is open to the objections which apply to each method, though in a much less degree. Still, even this combination is most unlikely

to yield so pure a product with such clean slags as are produced in our process, without a very considerable consumption of iron bars; for the solid fuel used for the reduction will, if that operation be carried far, inevitably reduce considerable quantities of iron to the metallic state; while, if it be not carried far, much copper oxide will pass into the slag in the subsequent fusion, and a heavy consumption of iron bars will be required for its recovery. With our process, on the other hand, the reducing operation may be prolonged until all the copper passes into the metallic state, since the reducing agent we employ is unable to reduce the iron oxides beyond the state of FeO; consequently if bar iron is used at all, only enough need be used to recover the copper which becomes oxidized during the fusion of the sponge, which, with proper care, will be trifling in amount. The consumption of fuel will not differ materially whether we use our process or the others we have been considering (*i. e.*, the reduction with solid fuel in the reverberatory, or the treatment of the fused ore with bar iron), but the labor will be considerably lighter in our process than in the others; since, in the latter, it is necessary to stir up and mix the reducing agent with the ore frequently, a most fatiguing operation.

In one of the cases (II, *a*) under which I have just stated that our process might frequently be applicable, its advantages are diminished by our generally having the comparatively inexpensive alternative of running down in cupolas the rich slags which would be produced if we attempted to obtain a rich product by the ordinary method. In the other two cases, however, in which our process is applicable (viz., I, *b*, sulphides plenty and fuels of class A unobtainable; and II, *b*, neither sulphides nor fuels of class A obtainable) this alternative cannot be resorted to, on account of the costliness or absence of fuels fitted for cupola use. Thus we may say in summing up, that our process should be especially useful when fuels fitted for cupolas are not to be had; and that its comparative usefulness will be greatly enhanced by the absence of sulphide ores and by the costliness of transportation.

A CATALOGUE OF OFFICIAL REPORTS UPON GEOLOGICAL SURVEYS OF THE UNITED STATES AND TERRITORIES, AND OF BRITISH NORTH AMERICA.

BY FREDERICK PRIME, JR., ASSISTANT GEOLOGIST OF PENNSYLVANIA.

THE first catalogue of Geological Reports of the United States was prepared by Prof. O. C. Marsh, and published in the *American Journal of Science and Arts* for 1867, vol. xliii, second series.

Since then the list has more than doubled, and it was thought that the publication of a new catalogue, brought down to date, would be useful to many of the members of the Institute of Mining Engineers. The compiler will be most thankful to have any omissions or inaccuracies in the list sent to him, to be published as a supplement in the next volume of the *Transactions*. Especially to have the correct title, number of pages, plates, and maps of any volume on the list to which an * is prefixed, as he has been unable to personally consult such works.

A preliminary list was issued last February, which has been almost doubled by the kindness of many friends. The writer desires more especially to express the obligations he is under to the following gentlemen: The Chief of Engineers, U. S. A.; Principal Dawson, Mr. G. M. Dawson, Drs. B. J. Harrington and T. Sterry Hunt, of Montreal; Mr. H. Engelmann, of La Salle, Ill.; Dr. F. V. Hayden and Prof. J. W. Powell, U. S. Department of the Interior; Prof. E. W. Hilgard, University of California; Prof. Henry Y. Hind, Windsor, N. S.; Prof. C. H. Hitchcock, of Dartmouth College; Mr. Henry A. Holmes, State Librarian, Albany; Maj. Jed. Hotchkiss, Staunton, Va.; Prof. W. C. Kerr, Raleigh, N. C.; Dr. J. S. Newberry, of Columbia College; Prof. W. H. Pettee, of the University of Michigan; Dr. Samuel H. Scudder, Prof. N. S. Shaler, and Prof. J. D. Whitney, of Harvard College; Mr. Sanderson Smith and Prof. J. J. Stevenson, of New York; Mr. Addison Van Name, of Yale College, and Lieut. Geo. M. Wheeler, U. S. Engineers.

PHILADELPHIA, 907 Walnut Street,
October 1st, 1879.

ALABAMA.

First Biennial Report on the Geology of Alabama; by M. Tuomey. Tuscaloosa, 1850. 8vo., xxxii, and 176 pp., and map.

*M. Tuomey, 1853. Geological Map of Alabama.

Second Biennial Report on the Geology of Alabama; by M. Tuomey, Geologist to the State; edited by J. W. Mallet. Montgomery, 1858. 8vo., xix, and 292 pp., and plate.

Geological Survey of Alabama. Report of Progress for 1874; by Eugene A. Smith, State Geologist. Montgomery, 1875. 8vo., 139 pp.

Geological Survey of Alabama. Report of Progress for 1875; by Eugene A. Smith, State Geologist. Montgomery, 1876. 8vo., 220 pp.

Geological Survey of Alabama. Report of Progress for 1876; by Eugene A. Smith, State Geologist. Montgomery, 1876. 8vo., 100 pp., and map.

ALASKA.

Report of the Superintendent of the United States Coast Survey for 1867. Washington, 1868. 4to. Contains, pp. 187–329. Report of George Davidson relative to the Resources and the Coast Features of Alaska Territory. Including Reports on Geology, by Theo. A. Blake; on Zoology, by W. G. W. Harford; and on Botany, by Albert Kellogg.

Geographical Notes upon Russian America and the Stickeen River; by William P. Blake. Washington, 1868. 8vo., 19 pp., and map.

ARKANSAS.

Geological Report of an Examination made in 1834 of the Elevated Country between the Missouri and Red Rivers; by G. W. Featherstonhaugh, U. S. Geologist. Washington, 1835. 12mo., 97 pp. and section.

First Report of a Geological Reconnoissance of the Northern Counties of Arkansas, made during the years 1857 and 1858; by David Dale Owen, Principal Geologist. Little Rock, 1858. 8vo., 256 pp., and 7 plates.

Second Report of a Geological Reconnoissance of the Middle and Southern Counties of Arkansas, made during the years 1859 and 1860; by David Dale Owen, Principal Geologist. Philadelphia, 1860. 8vo., 433 pp., 14 plates and map.

British America.

Henry Y. Hind. Report on the Explorations of the Country between Lake Superior and the Red River Settlement. Toronto, 1858. 8vo., 425 pp., and map.

S. J. Dawson. Report on the Exploration of the Country between Lake Superior and the Red River Settlement, and between the latter place and the Assinniboine and Saskatchewan. Toronto, 1859. 4to., 45 pp., and 3 maps.

*Henry Y. Hind. Report on the Assinniboine Expedition in 1858–9. Toronto, 1860. Folio, — pp., maps.

Report of Progress, together with a Preliminary and General Report on the Assinniboine and Saskatchewan Exploring Expedition. Made under instructions from the Provincial Secretary, Canada; by Henry Youle Hind. London, 1860. Folio, 219 pp., 7 maps, and 2 plates. [Blue Book.]

*Papers relative to the Exploration of British America; by Captain Palliser. [Blue Book.] London, 1859.—Maps, etc.

Report on the Exploration of British America; by Captain Palliser. Including Geological Report, by Dr. James Hector. 1857–60. [Blue Book.] London, 1863. 8vo., 325 pp.,—maps, plates, and sections.

Report of the U. S. Coast Survey for 1860. Appendix, No. 42. Notes on the Geology of the Coast of Labrador; by Oscar M. Lieber. pp. 402–408, and map.

British North American Boundary Commission. Report on the Geology and Resources of the Region in the Vicinity of the Forty-ninth Parallel, from the Lake of the Woods to the Rocky Mountains; with Lists of Plants and Animals collected and Notes on the Fossils; by George Mercer Dawson, Geologist and Botanist to the British North American Boundary Commission. Montreal, 1875. Roy. 8vo., xi and 387 pp., 2 maps, section, and 18 plates.

British North American Boundary Commission. Geological Report of Progress for the year 1873 (in part). Report on the Tertiary Lignite Formation in the Vicinity of the Forty-ninth Parallel; by George M. Dawson. Montreal, 1874. 8vo., 31 pp., and 2 plates.

California.

Geographical Memoir upon Upper California in Illustration of his Map of Oregon and California; by John Charles Fremont. Washington, 1848. 8vo., 67 pp.

Map of same.

Geology and Industrial Resources of California; by Philip T. Tyson. Baltimore, 1851. 8vo., xxxiv, 127, and 37 pp., 9 sections and 4 maps.

[Also printed, Washington, 1850, without xxxiv pp.]

* William P. Blake. Report to the War Department. Washington, 1854, 8vo., 80 pp.

Geology of the Sierra Nevada or California Range; by John B. Trask. ——, 1853, 8vo., 31 pp.

Report on the Geology of the Coast Mountains and Part of the Sierra Nevada, embracing their industrial Resources in Agriculture and Mining; by John B. Trask. ——, 1854, 8vo., 95 pp.

Report on the Geology of the Coast Mountains; by John B. Trask. ——, 1855, 8vo., 93 pp.

Report on the Geology of Northern and Southern California, embracing the Mineral and Agricultural Resources of those Sections, with Statistics of the Northern, Southern, and Middle Mines; by John B. Trask. Sacramento, 1856. 8vo., 64 pp.

*William Patton. Geology of a Part of Calaveras County. Dec. 1854. In Report to the Surveyor-General of California. Sacramento, 1855.

Observations on the Physical Geography and Geology of the Coast of California, from Bodega Bay to San Diego. Coast Survey Report for 1855. Washington, 1855. 4to., 23 pp. and 4 maps. [W. P. Blake.]

The Geological Survey of California, an Address delivered before the Legislature of California, at Sacramento, Tuesday evening, March 12th, 1861; by J. D. Whitney, State Geologist. San Francisco, 1861. 8vo., 50 pp.

Letter of the State Geologist relative to the Progress of the State Geological Survey. San Francisco, 1862. 8vo., 7 pp. [J. D. Whitney.]

Lecture on Geology, delivered before the Legislature of California, at San Francisco, Thursday evening, February 27th, 1862; by J. D. Whitney, State Geologist. San Francisco, 1862. 8vo., 33 pp.

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Annual Report of the State Geologist for the year 1863. N.d. 8vo., 7 pp. [J. D. Whitney.]

Lecture on Geology, delivered before the Legislature of California, at Sacramento, Tuesday evening, March 19th, 1863; by J. D. Whitney, State Geologist. Sacramento, 1863. 8vo., 17 pp.

Letter of the State Geologist relative to the Progress of the State Geological Survey during the years 1864–65 (erroneously printed 1863–64). Sacramento, 1866. 8vo., 14 pp. [J. D. Whitney.]

Letter of the State Geologist relative to the Progress of the State Geological Survey, during the years 1866–67. Sacramento, 1867. 8vo., 15 pp. [J. D. Whitney.]

An Address on the Propriety of continuing the State Geological Survey of California, delivered before the Legislature, Jan. 30th, 1868; by J. D. Whitney, State Geologist. San Francisco, 1868. 8vo., 23 pp.

Report of the State Geologist on the condition of the Geological Survey of California. Sacramento, 1869. 8vo., 7 pp. [J. D. Whitney.]

Letter of the State Geologist, relative to the Progress of the Survey during the years 1870–71. Sacramento, 1871. 8vo., 13 pp. [J. D. Whitney.]

Statement of the Progress of the State Geological Survey of California, during the years 1872–3. ———, 1873, 8vo., 14 pp. [J. D. Whitney.]

Geological Survey of California, J. D. Whitney, State Geologist. Geology. Vol. I. Report of Progress and Field-work from 1860 to 1864–1865. Philadelphia, 1865. 4to., xxvii and 498 pp., and plate.

Geological Survey of California, J. D. Whitney, Director. Palæontology, Vol. I. Carboniferous and Jurassic Fossils; by F. B. Meek. Triassic and Cretaceous Fossils; by W. M. Gabb. Philadelphia, 1864. 4to., xx and 243 pp., and 32 plates.

Geological Survey of California, J. D. Whitney, Director. Palæontology, Vol. II. Cretaceous and Tertiary Fossils; by W. M. Gabb. Philadelphia, 1869. 4to., xiv and 299 pp., and 36 plates.

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Mining Statistics, No. 1. Tabular Statement of the Condition of the Auriferous Quartz Mines in that part of Mariposa and Tuolumne Counties, lying between the Merced and Stanislaus Rivers, Aug. to Nov. 1865; by A. Rémond. 4to., 16 pp.

The Yosemite Guide Book. A Description of the Yosemite Valley and the adjacent Region of the Sierra Nevada, and of the Big

Trees of California. New York, 1868. Roy. 8vo., 116 pp., 28 plates, and 2 maps. [J. D. Whitney.]

The Yosemite Guide Book. Cambridge, 1869. 16mo., 155 pp., 2 maps. [J. D. Whitney.]

[Second Edition. 12mo., 186 pp., and 4 maps. Cambridge, 1874.]

Geological Survey of California, J. D. Whitney, State Geologist. Contributions to Barometric Hypsometry, with Tables for use in California. Cambridge, 1874. Roy. 8vo., 88 pp.

Supplementary Chapter and Practical Application of the Tables to the Observations of the years 1870–71, and a Discussion of the Results obtained; by J. D. Whitney. Cambridge, 1878. 4to., 24 pages.

Geographical Catalogue of the Mollusca found west of the Rocky Mountains, between latitudes 33° and 49° N.; by J. G. Cooper. San Francisco, 1867. 4to., 40 pp.

Catalogue of the Invertebrate Fossils of the Western Slope of the United States. Part 2. By J. G. Cooper. San Francisco, 1871. 12mo., v and 1 pp., 4–30 ff.

Geological Survey of California, J. D. Whitney, Director. Botany. Vol. I. Polypetalæ; by W. H. Brewer and Sereno Watson. Ganopetalæ; by Asa Gray. Cambridge, 1876. 4to., xx and 628 pp.

Map of the region adjacent to the Bay of San Francisco, in portfolio. New York, 1873. 2 miles = 1 inch.

Map of Central California, N.p.; n.d.

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† See also, under the heads of New Brunswick and Nova Scotia. Up to 1870 the Geological Reports were printed as an Appendix to the Journals of the Legislative Assembly in English and French. Only the English editions have been noted in this Catalogue.

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† Since 1867 the Surveys of Nova Scotia have been included in the Geological Reports of Canada.

*J. Howe. Report on Gold-Fields. Halifax, 1861. Folio, 7 pp. (Legislative Doc.)

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ROCKY MOUNTAIN REGION.

Message from the President of the United States communicating Discoveries made in exploring the Missouri, Red River, and Washita; by Captains Lewis and Clark, Dr. Sibley and Mr. Dunbar, with a Statistical Account of the country adjacent. Washington, 1806. 8vo., 178 pp., and folding title.

History of the Expedition under the Command of Captains Lewis and Clark to the Sources of the Missouri, thence across the Rocky Mountains and down the River Columbia. Performed during the years 1804-5-6. Philadelphia, 1814. 8vo., Vol. I, xxviii and 470 pp., maps. Vol. II, ix and 522 pp., maps.

Account of an Expedition from Pittsburgh to the Rocky Mountains. Performed in the years 1819–20, under the Command of Maj. Stephen H. Long; compiled by Edward James. Philadelphia, 1823. 8vo., Vol. I, 5 and 503 pp; Vol. II, 442 and xcviii pp., and atlas of 11 sheets.

*[Reprinted in London, 8vo., 3 vols., in 1823.]

Narrative of an Expedition to the Sources of St. Peter's River, Lake Winnipeck, Lake of the Woods, etc., under the Command of Stephen H. Long, Maj. U. S. A.; by William H. Keating. Philadelphia, 1824. Vol. I, xii and 439 pp. Vol. II, vi and 459 pp. With 15 plates and map in the two volumes.

[Reprinted, London, 1825. 2 vols.]

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Report of the Exploring Expedition to the Rocky Mountains in 1842, and to Oregon and North California in the years 1843–44; by Capt. J. C. Fremont, U.S. A. Washington, 1845. 8vo., 693 pp., 24 plates and 3 maps.

Notes of a Military Reconnoissance from Fort Leavenworth in Missouri, to San Diego in California, including parts of the Arkansas, Del Norte, and Gila Rivers; by Maj. W. H. Emory, U. S. A.

Made in 1846–7. Washington, 1848. 8vo., 416 pp., 41 plates and map.

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Journal of Captain A. R. Johnson, U. S. A. [Expedition from Santa Fé to San Diego.] Washington, 1848. 8vo., 48 pp.

[The last four are often found bound in one volume.]

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[Several of these Reports were printed separately.]

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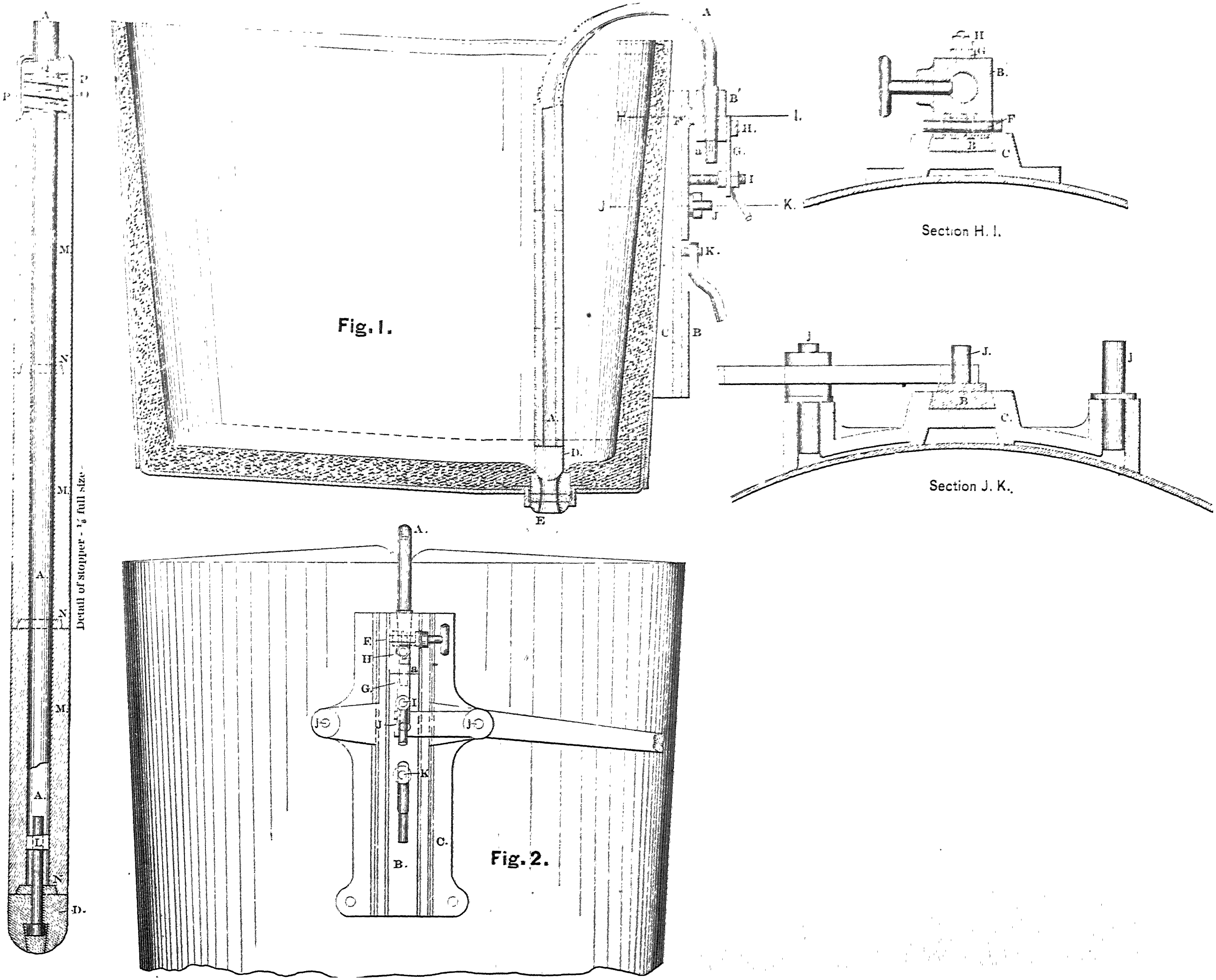
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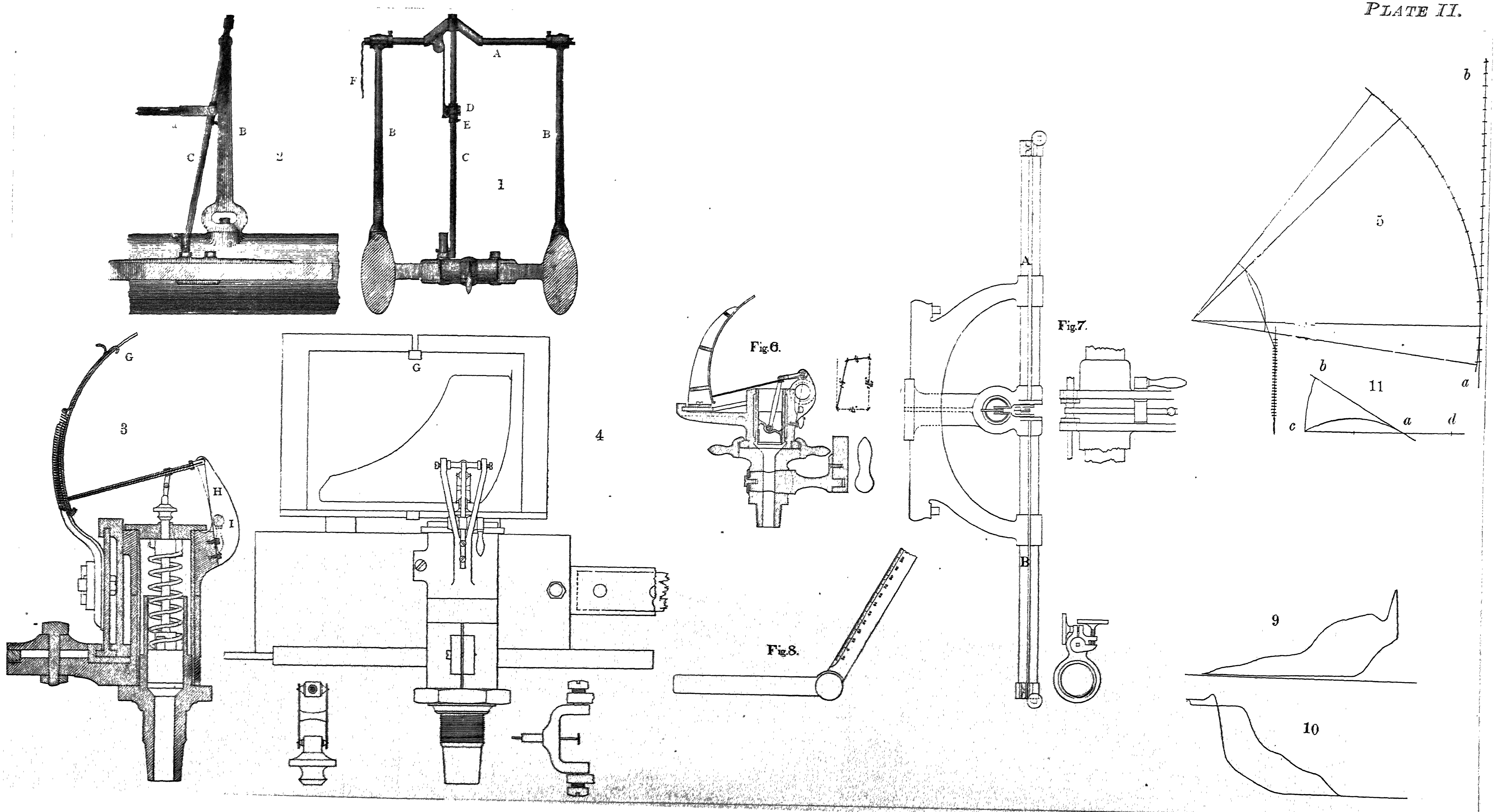

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